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STRUCTURAL FACTORS IN THE ANXIETY OF THE CHILD

BY

CARL FRANKENSTEIN, *Jerusalem*

1. ANXIETY AND THE DANGER OF EGO-LOSS

In most discussions of anxiety it is customary to distinguish between anxiety and fear [angoisse — crainte; Angst — Furcht (1)]. According to this widely accepted view, fear is caused by a threatening and dangerous factor, which lends itself to definition and identification in "objective reality," whereas anxiety arises owing to the operation of psychic factors and forces which have no definite correlates in outer reality. This distinction holds good even if we go on to say that sometimes anxiety "creates" reality-objects (which then serve as objects of fear) by way of projection of a psychic complex, such as in phobias, certain character-neuroses or psychotic conditions, and similarly in every other attempt at transforming the elements of the unknown, the irrational and the unconscious in anxiety into identifiable, rational factors. In other words: there are many fears which constitute the external form only in which anxiety appears.

The first critical question to be asked here is whether we can find a common denominator of anxiety and fear. Is it possible to discover, even in the most rational fear of the most tangible danger, some elements of anxiety? Can we in this way arrive at a determination of purely quantitative differences between anxiety and fear?

The anxiety-dream can perhaps be viewed as the "prototype of the human anxiety-situation," as it is a situation in which the dreamer appears as a helpless object of certain frightening "images" or "events" which *arouse* the anxiety-reaction (irrespectively of its essential causes). In the dream the ego ceases to function as the organising centre of consciousness. It becomes instead the object of autonomous processes which "happen". Here, then, anxiety appears as the expression of helplessness and fear of the absolute superiority of psychic elements which confront the dreamer in a concrete form. The intensity of anxiety seems to us the result of these two facts—ego-weakness and superiority of the unconscious. The real, concrete character of the images and their autonomous "actions" testify to the objective existence and the reality-character of a psychic force qualitatively different from the ego and stronger in creative power. Thus

viewed, the dream could only be compared to the creations of the unconscious in the psychotic which prove the objective existence of the unconscious as a psychic force much more convincingly than any neurotic symptom.

What does this mean for our attempt to understand the child's anxiety? The child, in the first stages of his development, is a helpless object of absolutely superior factors. His weak ego is incapable as yet of fulfilling its structural task as the organising centre of his limited consciousness. In the first years of life, the ego exists as a "nucleus" only, and the mere fact of the child being placed among adults puts in the centre of his life the experience of their absolute superiority in its positive and negative meaning. The adults constitute the objects of his relatedness and dependence, they appear as protecting and directing factors, but also as factors which limit and endanger his existence. The experience of this superiority becomes a central pattern of human experience and existence. It is true that this pattern changes in the course of time according to the quality of the child's life experience and the expansion of his consciousness; but throughout man's life it continues to "operate" as a constituent element of psychic existence and experience. When Freud stresses the traumatic character of the child's experience who witnesses the sexual act between his parents; when he makes the boy's readiness to fear his father, and the transformation of this fear into a general pathogenic anxiety, contingent on this first traumatic experience; when he speaks of the universality of the "castration-fear" in the child (resorting, as he does, to his theory of "archaic residues" to explain seeming exceptions) he exemplifies, in fact, the *existential* character of the child's anxiety, his fear of the adult's absolute superiority to whose arbitrary will he is helplessly subjected. This is one of the most important discoveries of psychoanalysis and one of its principal contributions to the understanding of child psychology. It is unfortunate that most psychoanalysts have drawn their examples of the experience of superiority and the resulting anxiety from the sexual sphere exclusively, limiting thereby the field of psychological research. Many other expressions of the experience of frightening superiority in early childhood have thus been ignored.

Ego-weakness and the experience of adult-superiority explain two principal fears in the young child: the fear of isolation and the fear of the dark. To be alone means to be deprived of the protector and to be forced, as it were, to live by relying on the weak ego-nucleus. If the child passes through a prolonged experience of isolation in the earliest period of his life, before the basis of inter-personal relationship has been laid, the

ego-nucleus may degenerate, and, as a result, his whole development may be stopped until the clinical picture of apathy, mental and motor retardation and absolute inability to establish interpersonal relationships comes into existence. *Such cases are characterised by absence of anxiety. We may therefore conclude that one of the essential conditions of anxiety is the existence of the ego as the subject of relation and relatedness to human surroundings.* This alone makes possible the emergence of fear of ego-loss in reaction to the interference of endangering factors, i.e. anxiety. A confirmation of this hypothesis is to be found in an analysis of another group of cases [studied by Bowlby, Roudinescu, Spitz, etc. (2)], in which the child was separated from his mother *after* the basis for interpersonal relationships had been laid (by contact with the mother). In these cases acute anxiety was found to develop (though the ways of its development and transformation were, of course, variegated, according to the interaction of constitutional and experiential factors). In the light of these facts we understand why every isolation through temporary loss of contact with the protecting environment is an anxiety-provoking danger-situation for the child. The intensity of the anxiety-reaction to loss of contact stands in direct proportion to ego-weakness, i.e. to the child's inability to live by relying on himself.

We have said that another fear characteristic of the young child is that of the dark. He needs sensory, at least visual and tactile, contact with his environment in order to maintain a minimum of identify-feeling: not to see the environment (nor himself), means that the world and the ego cease to exist or that the ego becomes an indefinite object of "something" unknown, which threatens *because* it is unknown. It seems that the importance of the visual connection as a means of discovering personal identity is for the young child whose senses are normally developed, greater than any other form of sensorial connection. (This does not, of course, contradict the well-known fact that tactile precedes visual contact, particularly in emotional and social development.) The process of gradual differentiation between object and subject which characterises the first stages of the child's development, is protracted enough; in the course of time it brings about a partial liberation of world-apperception from its sensory instruments, and thus leads to the self-perception of the ego as separate from the environment and as the active pole of the perceived world; but this process depends on a gradual extension of consciousness in general and of time-consciousness in particular. It then follows that in the first stages of development the loss of the child's visual contact with the principal representative of the ego, his body, is liable to constitute a

"danger-situation" and to cause anxiety. It goes without saying that the degree of inner security—resulting from the interplay of constitutional conditions with the environment's quietening or disturbing, strengthening or weakening influences—determines the intensity of the child's anxiety in a specific situation, the degree of his readiness to react with anxiety to various "danger-situations," and thus also the tempo of the process of overcoming anxiety-reactions. But the existence of individual differences does not preclude the analysis, identification and formulation of the structural elements in the individual phenomena.

However, as long as the weak ego feels itself protected by the superior adults from the danger of attack on the part of the unknown—anxiety will be less acute. Only if the weak ego feels itself *attacked and unprotected*, is the anxiety-reaction likely to occur: the significance of anxiety lies in that it constitutes an internal reaction to the danger which threatens the wholeness of the ego in a specific situation. This threatening danger is best known to us from the study of psychotic reactions, when the ego feels itself overwhelmed (suddenly or gradually) by the unconscious forces and contents which appear in the guise of obsessive ideas, as images, sounds, phantasies, etc., and when it is no longer capable of fulfilling its function of synthesis. But the danger that threatens the wholeness of the ego is no less present and real in the process of development itself, i.e. in all transitions from one state to another. It is only because development usually proceeds in small and slow steps that we do not fully understand the critical character of the very process of development, unless the essential difference between two stages is especially blatant (as, for instance, in adolescence). Structurally speaking, the process of development itself means giving up the present state, the actual and the familiar, the-protective-by-virtue-of-being-known. The horror novi, which is best being studied in the manifestations of personality disorder and ego-weakness, or in the crisis of the individual who is forced from a familiar into an unknown social situation, this fear structurally characterises the young child whose ego is weak and undeveloped, who finds himself, as it were, in a chronic state of transition, in transitional existence, without being able to relate himself consciously to the changing and transforming factors. The child is being transformed "from one state to another" regardless of his will, through the influence of internal and external forces which determine his development. The fact of his being an object of these factors and forces not only explains again why he sees the adult as an absolutely superior being and why he experiences the inner forces which transform him as absolutely valid and as what the adult would call "transpersonal"—

it also explains why the child's readiness to react with fear and anxiety to what happens inside himself and in his environment is immeasurably greater than that of the adult. We have already mentioned that only the gradual and organic expansion of the consciousness can help the individual to grow used to change as a structural factor of life. Only an expansion of consciousness makes him aware of the fact that transformation and transition do not endanger the very existence of the ego, and that everything new can become, and in fact does become, familiar through the transforming, assimilating and integrating activity of consciousness. As long as consciousness is not developed enough to carry out this task, every change caused by the developmental process is liable to turn into an experience of ego-loss (of losing the actual "state" of the ego), and thus—to arouse an anxiety-reaction. It follows, then, again, that the child's anxiety is a structural, and not a pathological phenomenon, and that its expressions cannot be interpreted or understood by an analysis of individual development only.

In other words: the factors which endanger the relative integrity of the child's weak ego (and thus arouse anxiety) are the factors of development *as such*, they are the forces which drive forward, the forces which develop (3). And the adults not only cannot ward off the danger of these forces (because it is they who represent them, through their demands and educational interferences), they are even bound to increase their terrifying intensity.

But the danger of losing the actuality of the ego-state comes not only from the developing forces, but also from the ever-ready regressive tendencies: a return to a previous state, too, means the loss of the actual state. We must therefore try to clarify at this point the significance of the concept of "regression" within the problem of anxiety.

We usually see the return to a previous state of development as a sign of recoiling from a defineable actual task, of ego-flight. The basic assumption of this theory is that the previous state to which the child (or the adult) returns is more pleasant than the actual one, and more suited to the executive and adaptive capacity of the weak (or hopeless, or insecure) ego. The difficulties which the ego encounters and from which it flees, are interpreted as difficulties in execution (performance) and self-realisation on a particular level of development, difficulties connected with the task of organisational consolidation of the ego's achievements, or with the task of transition to a different level of transformation. Thus the phenomenon of regression is here viewed exclusively from the standpoint of the ego, and it is interpreted finalistically, as though it had been proved that the

process of regression is a result of ego-activity. But just as repression is not a unidirectional process (repression of contents "into" the unconscious "by" the ego), but bidirectional (repression by the ego *and* attraction by the unconscious), thus also the process of regression ought to be interpreted as a bidirectional process: the ego's flight from reality, and the pull that the past exercises on the ego. This bidirectional nature of the process of regression explains also its frightening (anxiety-provoking) consequences which cannot be satisfactorily interpreted by any finalistic theory: Every previous stage of development, by virtue of its being a "previous" stage, is characterised by a relatively lower level of organisation (we are referring here to phases of growth in the normal child). Regression to such a level of lower organisation constitutes a threat to the ego insofar as the earlier stage has the power to dissolve the higher level of organisation already achieved, thus appearing as a heteronomous reality, superior to the regressing ego. The ego, therefore, cannot give up, and retreat from, its actual position in exchange for a previous, more primitive position, without at the same time giving up the relatively higher degree of autonomy obtaining on the higher level of development. Now, one of the elements and characteristics of "relative autonomy" is a greater degree of interconnectedness and integration of contradictory instinctual drives; whereas a weakening of autonomy means a renewed rise of isolated instinctual drives whose intensity is greater *because* they are disconnected and unrelated to each other (in violation, as it were, of the developmental law of integration). Thus, the child necessarily becomes a passive object of the instinctual forces of earlier stages to which he retreats, and of the images representing these forces. This is the reason for the strong anxiety accompanying every process of regression.¹

To sum up: we have so far suggested to explain anxiety (a) as a reaction to the aggressiveness of forces which threaten the wholeness of the ego by demanding development and forcing the ego out of its actual state into an unknown future; (b) as a reaction to the danger of the ego being attracted and swallowed up ("disintegrated") by forces of the past.

It would, however, be a mistake to conclude from the foregoing that these two dangers were clearly separated in reality (although we might be justified in speaking of the prevalence of one danger in a particular case).

¹ In "Hemmung, Symptom und Angst" and in other places, Freud mentions the danger of a disintegration (Entmischung) of the erotic and sadistic drives which is liable to result from a regression to a lower developmental level. This disintegration, he claims, should be viewed as one of the sources of anxiety resulting from regression.

Whenever the ego feels itself "drawn backwards" or exposed to the danger of being swallowed up by the disintegrating forces of the past, this feeling is accompanied by a fear of the demands posed both by the present and the future; whenever the child's ego experiences the forces demanding realisation and development as threatening his actual existence, this experience is liable to arouse a readiness to retreat, a readiness which necessarily puts in motion the heteronomous and anxiety-arousing attraction of the past.

We began our analysis of anxiety with a few remarks on the anxiety *dream*, since it seemed to us that the particular dream situation threw into relief the central structural element of anxiety—the helpless subjugation of the ego to the absolutely superior forces of the unconscious. Let us now briefly return to the question of anxiety-dreams and enquire into the function this "superior" unconscious has in their formation.

If our assumption is correct (that anxiety is the reaction of the weak ego to the danger of the aggressiveness of the forces impelling forwards and the danger of being pulled back), then we must assume that the contents of anxiety dreams, too, are related to these structural dangers; in other words, that the unconscious, being responsible for the formation of the dreams, relates to structural processes and the ego's reactions to them, and not only to the sphere of personal experiences (which according to our view function only as evoking, precipitating and specifying factors). Psychoanalysis tends to give to the anxiety-arousing dream-images a personalistic interpretation. If, for instance, the child dreams that a thief enters through the window, takes the toy-pistol which the child had prepared as a "defence" before going to bed, and threatens him with this pistol, which suddenly becomes bigger and more serious looking,—the psychoanalyst, connecting the details of the dream with the child's associations, interprets the thief as the image of the father who threatens the boy with castration. In another dream some other image may represent the threatening father, perhaps a policeman, a teacher or a dog. The anxiety-arousing threat, too, may be expressed in various forms, usually suited to the threatening image, and various objects may represent the penis threatened with castration. The mother, also, may be represented by the most varying images, human shapes, animals, monsters or certain features in nature; but in every case psychoanalysis tries to interpret the dream-contents by referring to the child's personal experiences and to concrete persons in his surroundings (especially the parents).

Why the child's penis is in one case represented by a pistol, and in another by a pencil; why the father appears as a thief in one case, and as

the teacher or policeman in another; why the mother appears once as a monster and once as a cave; why the danger of "being swallowed up" in one case assumes an aggressive guise, and in another that of passive drawn-ness—all these and similar questions, claims psychoanalysis, can only be answered with the help of the free associations of the dreamer. And, indeed, it is always possible to "explain" these differences in symbolisation by connecting the dream-contents with personal associations which refer to specific life-happenings, since their number is legion.

This method of interpretation is based on the assumption that as far as *contents* are concerned, there is no essential difference between the conscious and the unconscious, since both relate to the sphere of personal experience; it is true that by the process of repression of certain contents from the consciousness, the manner and quality of their interrelatedness change and, as a result, also their significance within the total personality; but their personalistic character remains unchanged. Hence the emphasis on the method of free associations aimed at calling back repressed contents to consciousness, and thus preparing the way for their reintegration within the sphere of conscious experience.

Psychoanalysis tends to explain both the anxiety-dream and the problem of anxiety in general by reduction to hidden or overt personal experiences; superiority causes anxiety *because* it appears as danger of castration by the personal father; external aggressiveness causes anxiety *because* it expresses the real danger of the father's revenge and punishment for the child's desire to kill him and take his place beside the mother.

As against this, Jung's analytical psychology starts from the assumption that the unconscious is not identical with the sphere of personal (repressed) contents, but includes the structural elements of experience, its "categories". These "categories" are represented—in dreams or phantasies, for instance—by images and events, phenomena and processes which may take on a personal or an impersonal *form*, but which always possess a transpersonal *significance*. Even the most "personal" images appearing in a dream can symbolise and concretise, as it were, the objective reality of life-forces; if we want to *describe* or *explain* these forces we use, of course, analytical language and abstract terms; but they *appear* in the dream most concretely and are autonomous vis-à-vis the dreaming person's ego and consciousness. We need the dreamer's free associations in order to understand his actual personal conflicts, through which a structural problem becomes crystallised in a specific case, but which by themselves cannot explain either the reality-character of the dream or its structural significance.

Thus, for instance, analytical psychology may say that even the child

who is afraid of castration—if such a fear is expressed in his consciousness, in his dreams or in his associations—is, fundamentally, anxious about the integrity of his personality which is constantly threatened by the world of the adults on the one hand, and by inner forces on the other (forces of development and transformation and forces of regression). But other fears, too, can express this existential anxiety about the integrity of the personality; the “choice” depends on the actual problem which preoccupies the child in his particular constellation and phase of development. This problem *may*, but need not be, fear of castration. And there is no justification whatsoever to *define* fear of ego-loss as castration-fear.

The differences in what constitutes the central problem in individual cases are also expressed in the variety of images which appear in anxiety dreams. A policeman is not a thief, and a thief is not a dog, and it is not accidental that one image appears in this case and a different one in another. We cannot discard these differences by reducing them—through making use of free associations and memory-residues from the foregoing day—to one personal complex. We must take seriously the differential significance of dream-contents, not only by connecting them with the personal associations of the dreamer, but also by considering their objective significance, by interpreting them as *specific* symbolic expressions. The dangers that threaten the weak ego's wholeness are numerous and various, the source of the danger may lie in organic processes or in the area of intellectual functions or in interpersonal relations or in emotional conflicts or elsewhere; the degree of activeness and passiveness in reactions and life-attitudes is different in every case and also changes from phase to phase. All these and other individual differences are expressed in dream-contents, and we cannot discover and extract their symbolic meaning by relying solely on the dreamer's associations. According to analytical psychology, the unconscious possesses the character of an autonomous force which uses, freely and as its purposes require it, all the individual's experiences in both their objective and subjective significance. The unconscious “knows” this objective significance even when the ego in its consciousness and cognition is still far from understanding it. This is the significance of the concept of “*collective unconscious*”, which is the sphere of the structural fundamentals or the “categories” of human experience. This level of significance exists even if the individual's free associations *do not* reveal it. Hence the reality-character, heteronomous for the ego, inherent in the creations of the unconscious, the dream-images and -processes.

Following these assumptions we suggest that the dream-images which

cause anxiety should be interpreted and understood as concretisations of *structural* facts and processes and not as direct or indirect expressions of certain personal conflicts. As we have noted above, the child's weak ego is constantly threatened by the danger of losing its identity by leaving an actual state in the process of development and transformation, and that, because of its weakness, it has no means of defending itself adequately against the attacks of the new and the unknown. We have also stressed the connection between the fear of transforming forces and their aggressive character, and the danger of regression and the threatening and heteronomous character of the forces that pull backwards. *These are the structural elements of anxiety which are expressed by the unconscious in the concrete form of anxiety-arousing dream-images.* It goes without saying that the form of the images and their actions are conditioned by the dreamer's specific cultural experience, since not every image can suitably represent the structural dangers in all cultural environments; but in every reality the unconscious will find the symbols it needs. We must also take into consideration the individual circumstances of the child's concrete situation in which he dreams, the specific realisation of general conflicts with which he is confronted. But the variety of concrete contents of the anxiety-arousing image's form and acts, does not detract from the basis significance of the anxiety-dream itself, which is: the inner reaction to the endangering of the actual state and the ego's integrity, a danger caused by the aggressiveness of the forward-driving forces and the pull of the forces of the past. (Only in passing we would like to mention here that the aggressive forces are usually symbolised by images of male character, whereas the forces which pull backwards are usually female in character.)

2. ANXIETY AND THE TWO LIFE-PRINCIPLES

So far it does not seem to be justified to conclude that there is any *essential* difference between anxiety and fear. The element of difference does not consist in the *experience* of the threat to the ego's integrity, but in the degree to which it consciously participates in the struggle with those endangering and threatening factors. But even if the ego participates to a larger extent in the rational fear of concrete and definite dangers, it cannot thereby ensure for itself a *full* autonomy in its war of defence. It will be enough to point to the affective element present in every fear and its typical physiological concomitants in order to exemplify the limited and partial nature of autonomy in a situation of "rational" fear; the limitation and partial character of autonomy shows that in the rational fear, too, fear of annihilation of the existing state is active, this element characteristic

of anxiety, which is fundamentally fear of death. The difference between the two affective states is quantitative; a greater degree of the ego's cooperation in fear makes possible a greater degree of rationalisation and the calling-up of auxiliary forces for the purpose of weakening and overcoming the fear. The ego usually mobilises these forces from other (non-affective) "zones" of the psychic structure, and it will be successful in proportion as the fear does not paralyse the ego's power of synthesis. The question therefore arises, what is the bio-psychological source of man's fear of the cessation of life. The paradoxical answer to this central question is: birth. We shall here confine ourselves to a discussion of the psychological aspects of the problem only.

Speaking of the "birth trauma" we must beware of the popular distortion of this idea, according to which the process of birth itself is accompanied by anxiety, and every anxiety which appears in the course of life constitutes something in the nature of a return to this original anxiety. This assumption is absolutely unfounded, and it will be enough to point to the fact that the appearance of anxiety is contingent on the existence of the ego, or at least a nucleus of the ego; and to assume the ego's existence at the moment of birth would be mere speculation.² On the other hand, however, we may see in the sudden transition from the vegetative existence of the foetus to the sensory form of life (on the basis of which the "cortical" existence develops) something in the nature of a biological shock which creates the *disposition* for anxiety-reaction. Some psychologists stress the biological importance of "oxygen-hunger" which, according to them, is a result of anoxia consequent upon the sudden transition from the vegetative to the autonomous mode of existence. It is to be doubted, however, whether we are justified in speaking here of "oxygen-hunger"; it might be more correct to speak of the physiological difficulty of *introducing* the necessary quantity of oxygen, a difficulty which grows less and less in proportion as the respiratory apparatus adapts itself to the needs of autonomous life. But whether we accept the hypothesis of anoxia as the traumatic basis of the process of birth or not—still the sudden transition from the protected vegetative existence of the foetus to the autonomous existence of the newborn undoubtedly constitutes a primordial biological shock which puts its stamp on the very autonomy of the individual's existence. It has been said that the process of birth is a process of

² Compare also Freud's criticism of O. Rank's theory of the birth-trauma, e.g. in "Hemmung, Symptom und Angst".

"resurrection after the foetus' death." This formulation emphasizes the element of discontinuity between the two forms of human existence.

We should like to develop these ideas in a slightly different direction. The foetal existence is "existence in narrowness" as against the unconstricted existence of the newborn. The transition requires an adjustment of physiological functions which usually takes place in the first days of life. If the anoxia-hypothesis is correct, then it is possible that the original "feeling of constraint" which, according to this hypothesis, occurs at the moment of birth, bridges, as it were, the foetal existence with that of the newborn, though its character changes at the same time from a positive one (protection) to a negative one (deficiency); it is possible that here lies the basis for the reflectory connection in later life between the experience of "lack of . . ." and the tendency to seek protection, a tendency prepared, as it were, in the prototypical birth situation, although it clearly appears only at a later stage of development³; it is also possible that we can see here the basis for three principal forms of reaction to the experience of existential "narrowness" (i.e. to anxiety): regression to the former state (of being protected), a real or imaginary flight to the protector, and psychic paralysis in its various forms (such as petrification, fainting, aimless hiding etc.). In this respect we might see in the fact of narrowness at the moment of birth the biological basis of the experience of anxiety.

The question now arises what is the *psychological* significance of the process of birth and the first stages of development after birth. The "feeling of narrowness" arises precisely at the moment in which the contact between the newborn and the mother is interrupted. The first physiological means for overcoming anoxia is, as we know, the cry. There is no need to discuss the absurd assertion that "the baby starts its life in anger"; but we can undoubtedly say that the cry *changes* from an expression of biological needs into a socio-affective expression insofar as it helps the child to bring back those who care for him, especially the mother, and thus to re-create, though only partially, contact and unison with her. Thus the "physiological cry" becomes related to the tendency of "returning to the mother", and thus the patterns of tactile satisfaction, which take the place of a full return to the womb, come into existence (bringing about also the establishment of the first interpersonal relationships).

We have already mentioned isolation as one of the principal conditions for the appearance of intense anxiety in the child, since in a state of

³ cf. S. Freud, *op. cit.*

isolation the danger of being attacked by images representing the autonomous forces which threaten ego-integrity is greater. This assumption should here be supplemented in the light of the significance of the baby's contact with his mother—his basic pleasurable feeling.

It is doubtful whether, phenomenologically speaking, "pleasure" should be defined as "elimination of negative stimuli" (satisfaction of impulses or elimination of tensions), since in every experience of this kind, at least after the emergence of the ego-nucleus, a clear or vague consciousness of the temporariness of satisfaction and its dependence on external factors beyond the sphere of the ego is present. Rather, the experience of staticness, completeness and knownness, i.e. the experience of absolute balance, would seem to be the source of the genuine feeling of pleasure, because it makes the illusion of eternity possible. The individual reacts to an interruption of this pleasurable feeling (following the intervention of change-factors) by endeavouring to mobilise the forces required for the re-establishment of the previous, staticness. (Thus, it may be said that even the regressive tendency and not only resistance to change is a derivative of the tendency towards staticness, i.e. of the pleasure-principle.) A failure of this tendency towards staticness creates a feeling of discomfort, a feeling arising, of course, also from the very experience of tension or the disturbance of inner balance. But there is a fundamental difference between the two: the discomfort which stems from the ego's inability to return to the state of completeness and staticness, is likely to give rise to a feeling of helplessness and thus of anxiety, whereas the discomfort accompanying the very experience of tension is liable to cause a variety of reactions—withdrawal, apathy, aggressiveness, restlessness etc.—but only in extreme cases anxiety.

In the earliest stages of infant life, however, the two basic forms of the pleasure principle, the tendency towards staticness (through remaining in, or returning to, a "known" situation) and the tendency towards elimination of tensions, are still merged into one, the latter being the main expression of the former, and the former supporting and actuating the latter. The *experience* of staticness is the source of the *striving* after pleasure; the latter expresses itself first and foremost in the infant's tendency to remain in constant tactile contact with the mother (including, but not identical with, oral satisfaction). Thus, the mother establishes herself as the primary image of staticness. To the extent that the child—owing to positive environmental and constitutional conditions—succeeds in obtaining satisfaction of his needs (and mainly through contact with his mother), an ever-growing experience of staticness *in* the process of development leads

to the formation of what Erikson calls the "sense of trust" (4), which, teleologically speaking, constitutes a structural "weapon" against the danger of anxiety. If, on the other hand, the child *fails* in his tendency towards elimination of tensions—owing to negative environmental and constitutional conditions—there is liable to occur what might be called a structural "perversion" of the tendency towards staticness: identification with the known will be substituted either by a compulsive or terrified clinging to some (at least partially pleasant) present, or by apathetic "acceptance" of its traumatic nature; and the place of natural contact with the protecting past (the mother) will be taken by a tendency to avoid or to flee from the present, by a heteronomous retreat or "fall" into the past. (Here becomes evident the decisive importance of education in the earliest stages of development, in which the basic tendencies of the individual's psychic structure are being shaped.)

But since change is a biological fact, and the loss of staticness therefore inevitable both biologically and psychologically, any monistic interpretation of development aimed at explaining the facts of life by postulating the pleasure-principle or the tendency towards staticness as the one and only structural law, is bound to fail. The fact of growth and development requires the assumption of a second basic life-tendency which we suggest to call the tendency towards expansion, and which is structurally opposed to the tendency towards staticness. The contradiction between the two tendencies constitutes the dialectic basis of life, and the structural tension between the two is the core of any dynamic theory of development. Psychoanalysis speaks of the pleasure-principle versus the reality-principle, the latter being almost identical with the tendency towards adjustment which is thus conceived of as essentially connected with the experience of discomfort or displeasure. In this conception, life and the individual are viewed from a social rather than an organismic point of view. It would seem that it is this one-sidedness of orientation which explains, at least in part, the strong valuational tendencies as well as the pessimistic undertone in Freud's philosophy of life which found its most striking expression in his later attempts to replace his earlier metapsychological theory by his more biologically oriented Eros-Thanatos speculations (5).

In our definition of the basic dichotomy of life as structural polarity of the tendency towards staticness and the tendency towards expansion, pleasure and displeasure become secondary only, characteristic of certain structural constellations of the two basic life-principles rather than of the one and the other in isolation. To give an example: The structural tendency towards expansion (growth, development, change being its manifestations)

is not *identical* with the reality-principle in its socialisation aspects, but it is being *used* by education for its purposes. In this way, educational activities and environmental demands become, for the child, linked to the principle of expansion and development in its biological sense. Now, to the extent that constitutional and environmental conditions make possible a (relative) integration of the two basic principles of life, to that extent will growth, change and adjustment (resulting from organic processes as well as from educational intervention) be accompanied by feelings of pleasure. If, however, the two principles are *not* operating in (relative) integration, every process of transformation is bound to be accompanied by feelings of displeasure and tension.

We maintain that the contradiction and tension between the two structural principles is an essential, "constitutive" factor of life, and that, in a normally developed individual, the two principles are organically and constructively interwoven. A recognition of this organic interrelation may contribute towards the understanding of our problem of anxiety.

We have suggested to use the terms "principle of staticness" (wholeness, balance) rather than "pleasure-principle" and "principle of developmental expansion and transformation" rather than "reality-principle". In the actual life processes, we may now add, these two principles appear in mutual interrelatedness only: the first as *tendency* towards staticness, disturbed by the tendency towards expansion; and the second as *tendency* towards the formation of new states, i.e. as the tendency towards expansion and transformation, subjected to the inherent organisational aims of life. In opposition to Freud's metapsychological concepts we argue that the aim of life is the creation of complex organic and psychic systems out of simpler units. This creation goes through two phases: expansion and systematisation. The process of expansion and transformation can, it is true, be achieved only through the relative negation of what exists; but as long as this negation is subject to its structural aim (integration) it does not contradict the second life-tendency, that towards staticness, since this tendency, too, is normatively expressed in actual life only as a tendency towards the *formation of new systems* and never, because of the eternal processuality of life, achieves its aim. The libido feeds both life-tendencies equally. This, however, means that the tendency towards staticness is *not* identical with the striving of the organic towards death, and the tendency towards expansion is *not* identical with the "erotic tendency" to continue life; *both function by interaction in order to achieve the life-aim which is the formation of complex systems more highly integrated*. Both tend, like every organic or psychic process, towards the elimination of the separate-

ness of elementary units, the second by way of *polarisation* and the first by way of *systematisation*. Polarisation means: elimination of separateness through relating contradictory elements to each other, thus preparing and making possible the coming into existence of new systems; systematisation means: elimination of separateness through forming new systems from mutually related elements. Only when the two tendencies function in isolation, without interaction, does the tendency towards staticness degenerate either into regression or into "petrification", (under the direction of a perverted pleasure-principle, as it were); and the tendency towards expansion becomes a destructive and aggressive tendency towards a blind conquest of life (under the direction of a perverted reality-principle, as it were).

Life admits of no resolution of the essential tension between the two principles either by means of fleeing into staticness or by means of aggressively negating an existing actuality; the tension can only be resolved by *acquiescence in the process of transformation accompanied by a tendency towards systematisation, towards the formation of new systems*. If the child's mental and physical functions are normally developed and if education fulfils its normative function, the child is likely to attain an experience of security through becoming accustomed to the rhythmical regularity of change from tension to relaxation, from transformation to staticness, and thus, also, to experience the temporary character of the disturbance of balance and staticness. Such a child will be able to put up with the "stage" between leaving a known present and entering into a new present as a necessary transition. (In Freudian terms: he will learn to bear discomfort without fear, and thus the pleasure-principle will help him to realise the reality-principle and the reality-principle will gradually become the actuator of the pleasure-principle.)

But where this normative process is disturbed, whether by constitutional or experiential factors, anxiety is liable to appear. Here we agree with F. Rothschild's definition of anxiety as an affective sign of the separation between the two life-principles: anxiety appears when the tendencies towards staticness and towards expansion function separately, without mutual relatedness (6), when, as we said, the first is liable to cause regression or petrification and the second destructiveness (directed either inwards or outwards, but actually, as we shall see later, both ways). *It is this separation which makes possible the experience of being in danger of ego-loss* (and which could therefore be defined as the structural condition of anxiety); for, in the last analysis, this danger can be reduced to, and

explained as, a structural deficit in the power of growth and of systematisation.

In other words: the actuality of the danger of ego-loss resulting from the forces of development (striving forwards) and of regression (keeping back or pulling backwards) depends, ultimately, on the degree of integration i.e. of mutual relatedness of the two life-principles or, more specifically, on the degree of *lack* of integration, of separation between the two principles. The existential definition of anxiety as an expression of the structural tension between the two life-principles from which we started out in this analysis, is incomplete, because it takes into account only the one element of the configuration, the structurally given tension. It must be supplemented by the second element of the configuration, the individual experiencing life, or: the degree to which the individual resolves the structural tension through bringing the mutually opposed life-principles into interaction. It is only by means of such a configurational approach to our problem that we shall be able to answer the question of individual difference: *what* forces of development, regression or staticness constitute a danger to the ego, is determined not by the fact of structural tension alone, but also and mainly, by the individual degree of integration. But in order to make this individual degree of integration meaningful for the understanding of the essence of anxiety, the hypothesis of the two basic life-tendencies should, in turn, be supplemented by a theory of the structure of the psyche, in other words, of the dynamic relationships between the ego as the center of consciousness and the unconscious, or, as we would prefer to say, the total psyche.

Following Jung's distinction between the personal and the transpersonal areas and contents of the unconscious, and his concept of the total psyche of which consciousness is a late derivate only, we maintain:

(a) that ego-formation presupposes both a process of self-limitation of the total psyche and a relative and temporary separation of the ego from the total psyche (called also the process of adjustment or the "conquest" of reality);

(b) that this process of separation is both a manifestation of the life-inherent tendency towards systematisation and a structural necessity for the second life-tendency, that of expansion, which ultimately is identical with the dynamic character of the total psyche, i.e. with its tendency to realise itself *through* the ego as its individual reality-organ;

(c) that, on the other hand, the ego's organisational function and activity (and its systematisation) which we structurally interpret as expressions of the life-tendency towards staticness, are, in the last analysis,

normative expressions not only of the total psyche's tendency towards self-realisation, but also of the ego's readiness to change, to grow, to expand under the direction, as it were, of the total psyche;

(d) that this normative constellation can be disrupted by the very fact of the ego's relative autonomy vis-à-vis the unconscious or, better, the total psyche; that this autonomy, although structurally necessary for the ego to fulfil its function as regulator and "transformator" between the unconscious and outer reality, explains the existence of various "degrees of contact" between the ego and the total psyche, ranging from complete openness to complete isolation, from unlimited readiness for change, growth and expansion to complete identification of the ego with its actuality;

(e) that we must, in addition, distinguish between the various degrees of the ego's power to organise, synthetise and systematise both its actual functions and contents of consciousness and the functions and contents accruing to its area of competence in the process of change, growth and expansion (not to speak of such other differential factors as constitutionally or experientially determined degrees of activity or passivity, extravert or introvert attitudes, intelligence, extent of consciousness, degree of development and intensity of ego-functions, relation between superior and inferior functions, etc.);

(f) that organisational strength in the ego on the one hand, and a high "degree of contact" (i.e. relative openness towards the total psyche) on the other, are the preconditions of normalcy; whereas a deficiency in one respect leads to such pathological manifestations as: rigidity, aggressivity or self-centeredness in persons with organisationally strong egos which are shut-off (isolated) from the total psyche; fears, anxieties, obsessions or hysterias in persons with organisationally weak and shut-off egos; phenomena of "inflation" or "invasion" in persons with organisationally weak but relatively "open" egos, etc.

Thus we see that the problem of mutual relatedness and integration of the two basic life tendencies, that towards staticness and that towards expansion (which seemed to us essential for the understanding of anxiety) must be viewed as a problem of ego-constellations. After having defined anxiety as a reaction to the danger of ego-loss, we can now formulate the following "laws":

(1) The danger of ego-loss is directly correlated to weakness of systematisation in an ego which, on the other hand, is relatively shut-off from the total psyche. In this case, the ego is, paradoxically, exposed to the (uncontrolled) influx of non-ego forces and contents, to what might

be called "expansion from without." Such a constellation makes the experience of staticness impossible, and, as a result, the ego's structurally weak power of organisation is bound to grow still weaker and its heteronomous exposure to the non-ego correspondingly acuter. (We should bear in mind that the non-ego includes in this case not only the area of the unconscious but also that of the social reality which is bound to become the representative of the former to the extent that the ego loses or fails to attain autonomy by failure to relate itself to the non-ego.)

(2) Isolation of an organisationally strong ego from the non-ego may lessen the danger of ego-loss and thus the emergence of anxiety, although the rigidity characteristic of such a constellation limits the possibilities of growth. In this case, the danger to the personality-structure is not so much in the nature of a constantly present threat to the ego but rather one of a sudden and total breakdown which is liable to occur when the ego's organisational strength is hollowed out by the negative force of stagnation.

(3) The danger of ego-loss and its concomitant anxiety are, as we said, proportional to the ego's organisational weakness. If, however, this weakness is so extreme (as the result either of constitutional factors or of early life-experience) that the ego almost ceases to fulfil its protective, regulative and synthesising functions (e.g. in certain types of mentally retarded or apathetic children), anxiety becomes structurally impossible—to the extent that the ego is not any longer there to be lost, as it were. (This is only another way of saying that anxiety is liable to be more intensive, the more differentiated a person is.)

(4) Ego-extinction in certain forms of psychotic withdrawal or ego-inflation in psychopathic states are extreme examples of what might be called a perversion of the two life-principles, examples of a pathological disconnection between ego and total psyche. These cases are therefore characterised by the absence of anxiety. They should, however, not be confused with other cases of extreme pathology, in which the ego is constantly threatened with extinction by forces invading it heteronomously. The ego, in order to experience anxiety, must be able to feel or to know itself both as a subject and as object of a superior non-ego.

3. ANXIETY AND FEAR

Now we can return to our thesis that fear differs from anxiety in the degree of ego-participation only, but not in essence. According to this criterion of ego-participation, neurotic fear stands between the normal fear of "concrete" dangers in external reality and the anxiety which is liable

to be provoked, for instance, by the manifestations and creations of the autonomous unconscious; and we can understand why these differences are blurred in the child, whose fears are not only more numerous than those of the adult but also more irrational. Freud emphasized the similarity between the normal child's fears and neurotic fears, and asked himself why most of these fears disappear in the course of development without leaving any traces of psychic disturbance, but he admitted he could give no satisfactory answer to his question (7). We suggest to start from the assumption that what constitutes an irrational and objectively inadequate fear *in the adult*, may be, *precisely because it is an irrational fear*, adequate to the child's reality. The child's world of experience is characterised, as we know, by the concretistic character of his perceptions, his thinking and his imagination; his world-pictures do not fit the "objective" significance of reality or, better, they fit the *inner* and not the outer reality. Therefore, fear aroused in the child, under certain conditions, by this inner reality and the concrete images representing it, is bound to be irrational (irrational from the standpoint of the adult and his conceptual world); but we should beware of concluding from this fact that we are faced here with pathological deviations. The degree of ego-participation is weak in the child *qua* child, and only when it sinks below the level which is normal and suitable to his stage of development, are his normal fears, aroused by images representing his inner reality, liable to become neurotic fears. If, however, development proceeds normally, then the irrationality of the child's fears disappears in proportion to the enlargement of his consciousness, which means expansion of outer reality at the expense of inner reality. In other words: irrationality must be viewed, in the context of the anxiety problem, as a developmental symptom rather than as a factor of definition. The fact that the irrationality of children's fears normally disappears with age is a proof for the developmental process of integration of the life-tendencies, that towards staticness and that towards expansion, of the ego's growing ability to systematise change and to change its systematisations. On the other hand, if we look closely at the adult's most rational fears (and even the child's, insofar as such fears arise in him already), we can find at the basis of all of them the anxiety-elements of an open or veiled fear of death, or fear of a shrinkage (i.e. partial destruction) of the ego resulting from failure in a definable reality-situation or from aggressiveness on the part of superior forces. These fears will be the more irrational, the more certain developmental or experiential factors succeed in disrupting the (relative) integration of the mutually opposed life-tendencies already attained, thus impairing the ego's (relative)

autonomy vis-à-vis the non-ego. Then, the structural (normal) anxiety-elements will again be re-inforced and intensified as the result of a (relative) re-establishment of heteronomy which, of course, increases the danger of ego-loss. Hence, anxiety is more pronounced in neurotic (and still more in psychotic) fears than in normal fears, in which the ego remains a (relatively) autonomous partner of objective reality. We must, however, remember that a similar shrinkage of autonomy may also come from the outside, as it were, that is from extreme reality-dangers, and then intense anxiety will appear no less, or perhaps even more, than in neurotic or psychotic states, and may even bring about an absolute paralysis of the ego.

4. EDUCATIONAL CONCLUSIONS

Although anxiety can never be eliminated, it can well be alleviated by the educational environment. In this context, the function of education could be defined as developing, strengthening and protecting the ego's ability and readiness to "accept" expansion, growth and development as the natural correlate of the structural tendency towards systematisation. The basic trust resulting from the feeling of being protected and from the experience that disruption of staticness is temporary only, prepares the child to bear unpleasantness without fear and makes possible relative integration of the two life-principles. It is, to use E. Erikson's terms, the precondition of autonomy which, in turn, makes possible the gradual "conquest" of reality and, later on, the establishment of a sense of identity. What are the educational conclusions to be drawn from these theses?

The dialectic basis of our problem comes to light when we remember (a) that the adult constitutes for the child the source both of anxiety and of protection; and (b) that a strengthening of consciousness by means of education is liable both to weaken and to intensify anxiety.

As regards the first contradiction, we need not add much to what has been already said: every individual—but especially a child—needs courage in order to accept change of his actuality and expansion of his consciousness, a courage the development of which depends no doubt on many constitutional factors but still more on that basic sense of trust which the child acquires in the first years of his life through stable relationships, warmth of feeling and empathy on the part of his parents and all those who are concerned with his upbringing. The ensuing feelings of confidence and security which make it possible for the child to experience

staticness, should be viewed not only as the *result* of the child's trust in his environment, but also as the factor which further strengthens his trust not only in the environment but also in the inner forces of development to which such a child gradually comes to "entrust" himself without anxiety. Thus, the adult may become a positive protective force which can be relied on as long as it does not impose itself from without, but protects and guides so that the ego may be able to grow and develop from within.

Here, however, the first contradiction is connected with the second. Development is viewed here as the process of consciousness-expansion through the ever clearer differentiation between the ego and the world, between the animate and the inanimate, between being active and being acted upon, between the actor and the action, between cause and result, between the familiar and the unknown. Yet, even though this separation (differentiation) constitutes the *conditio sine qua non* of ego-development, it is not identical with it. Synthesis, reorganisation and systematisation are the processes and acts which give constructive meaning to the expansion of consciousness in all stages of development and for every psychic and organic function. It follows that each one of the two stages of consciousness-expansion (separation and systematisation) has a different affective meaning: the first is a source of anxiety (like every process of separation), whereas the second is a source of allaying anxiety (like every return to relative staticness).

The task of education, then, is to turn the negative experiences of adult-superiority and of separation in the process of consciousness-expansion into the positive experience of stability which makes the emergence and the preservation of trust possible and prepares the ground for the expectation of new and more complex units of inner and outer reality emerging from separation and change. It is in this way that, what psychoanalysis calls the pleasure-principle gradually becomes integrated with the reality-principle: the experience of the stability and the unequivocal meaning of the adult-world establishes its character as a protecting instead of a threatening reality; the process of getting organically accustomed to the unknown makes possible the experience of systematisation as a regular sequence to change. When we speak of the normative significance of the child's identification with "his" adults, we are really saying that the child gets a chance to project himself, as it were, into a future represented by his objects of identification, and to anticipate unconsciously (and—later—consciously) the state which will *follow* his actuality. Hence the vital significance of the parents for the child and the process of his gradual relative "conquest" of anxiety, a process which is possible only when the

child's unity with his parents as symbols of protection and guidance is ensured.

But in spite of the decisive causal function of the child-parent relationship in the process of solving the anxiety-problem and preventing pathological deviations, the impact on this process of specific methods and actions should not be underrated. In the following, some of the most important methods are mentioned (each of which takes, of course, a different form in each developmental stage of the child and in each socio-cultural sub-group).

(1) Expanding consciousness through knowledge and understanding of facts aimed at, and resulting in, reality-orientation.

(2) Strengthening rationality.

(3) Giving the child opportunities for expressing his anxiety (instead of suppressing it).

(4) Relating anxiety to "personal" (concrete) forces and factors in order to enable rationalisation on the one hand, and as natural a contact as possible with the unconscious on the other.

(5) Developing courage in dealing with transpersonal life-tasks which demand readiness for change.

(6) Strengthening the ability to establish satisfactory social relationships by means of directive education on the one hand and by the experience of group-reality on the other.

These, however, are not *alternative* means; only a full integration of *all* of them into an unequivocal educational method can ensure their effectiveness as tools in the hand of an educator, who wishes to help the child in his struggle with anxiety and to prevent pathological deviations. Every teacher knows that enlarging the extent of consciousness will promote reality-orientation only if accompanied by a strengthening of rational understanding; but even the promotion of this orientation will not help the child deal with his anxiety problems effectively if he is forced at the same time to suppress its expressions. On the other hand, expressing anxiety by itself will not help either, unless the child gets a chance to concretise, as it were, the anxiety-arousing images and turn them into objects to which he can relate himself personally and quasi-rationally. Thus, the child will learn how to "communicate" with his unconscious without losing the beginnings of ego-autonomy. And yet, even this means will not help unless the educator succeeds in developing in the child (organically and in a way suited to the different stages of his development) the courage necessary for taking an active and adequate interest in the new and unknown, for trying it out and directing his activity towards it. The

tasks which afford an opportunity to acquire this courage belong to all reality-spheres, those of perceptual, practical, emotional and social experience. Without it, passivity is likely to prevail in the development of the child's personality, and passivity in turn is liable to weaken the power of rationalisation and thus again to intensify the danger of anxiety.

Education based on such principles means education for development, for overcoming the fear both of death and of the new. Ego-strengthening diminishes the danger of feeling attacked since it furthers autonomy, i.e. the ego's ability for reality-orientation and for enlarging its conscious bounds by perceiving the possible and the potential; and the ability to form complex systems out of more elementary ones, through conceptual and evaluational connection of life-experiences.

But education towards (relative) liberation from anxiety would be incomplete and, indeed, meaningless, without the simultaneous development of the ability to establish satisfactory inter-personal and group-relationships, to live in social contexts. This ability is both the prerequisite and the result of the transformation of aggressive (threatening) reality-forces into ego-supporting and strengthening factors: the higher the degree of integration, of mutual relatedness of the two basic life-tendencies, the stronger will the individual's courage grow not only for change, growth and transformation, but also for the establishment of relationships with other individuals. The wider the scope of genuine social experience, the better will the individual's chances become for the development of his ability to establish relationships and to grow through them. Social education which gives the child such experiences (in dynamic groups as well as in personal relations) therefore furthers the process of structural integration, from the periphery as it were, and, as such, is an indispensable and integral part of any educational action.

In this way, education performs an important preventive function, though necessarily of a partial and relative character only: while no education will ever be able to eliminate the structural tension between the mutually opposed life-tendencies, it can and indeed should help in the formation of an ego capable of relative integration, of bridging between expansion and systematisation, between growth and organisation, in other words capable of fulfilling its regulative function as a transformative function. This means that the child must gradually come to experience "danger" as "danger from within", manageable from within (although he will long continue to experience both the "perils of the soul" and the protecting forces by way of concretisation, in the form of threatening

and protecting images.⁴ Development, too, must become an inner process, transformation of the identical though changing self, rather than change imposed from without. These are the conditions of gradual (and relative) liberation from anxiety; to create, develop and safeguard them is the ultimate aim of education.

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⁴ Processes of imaginal *concretisation* should not be confused with processes of projection, i.e. with *personalisation* of anxiety factors: it is by *eliminating* projective personalisations that the hostile character of the adult's world gives way to its protective character.

LEVELS OF MUSCLE ACTIVITY IN PSYCHIATRIC PATIENTS *

BY

IRENE MARTIN

Institute of Psychiatry, London

The literature on the relationship of muscular tension to concepts such as "anxiety" and "emotionality" has steadily increased in volume and scope during recent years. Many of the earlier experimental studies employed indirect methods of measurement such as pressure of the hand on a bulb, force of movement during the performance of a motor task, grip pressure, and involuntary tremor (4, 5, 10, 11, 14, 18, 25). Observational methods employing rating scales (28) and time sampling techniques (24) have also been used. Davis (2) has implied, however, that the interrelationships of different indices of tension are largely unknown, and he has supported the direct recording of action potentials from muscles.

Such direct electromyographic techniques have since been used extensively by Malmo and his co-workers (19, 20, 21) and Sainsbury and Gibson (26). All these authors took recordings from psychiatric patients, as well as normal subjects. Malmo recorded from neck and forearm muscles, and found that psychoneurotics, although similar to normals in level of tension during rest, responded to various stressful situations with a greater innervation of the skeletal musculature than the normals. On the other hand, Sainsbury and Gibson, who recorded from right forearm and frontalis muscles, found marked significant differences between selected anxious neurotics and normals during relaxation. However, more recently Malmo and Smith (23) have reported confirmatory findings of this rest-period frontalis difference between neurotic patients and normals.

Malmo and Shagass (19) also obtained recordings from psychotic patients, and found that early schizophrenics were as hyper-reactive as neurotics with severe anxiety symptoms.

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In view of the need to establish more positively those situations in which discriminative levels of tension between psychiatric patients and normals are found, the present research was designed with the following aims:

(a) to compare the level of muscle tension obtained from three groups—neurotics, psychotics, and normal controls. The neurotic group was split, for further analysis, into two sub-groups comprising hysterics and dysthymics;

(b) to check whether differences in levels of tension of the groups described above can be found during relaxation, and/or when the subjects are “stressed” by situations which will be described later.

In addition, information was sought concerning possible differential rates of relaxation between the groups. Other aspects of the data to be considered were (a) concerning the degree of consistency found in levels of muscle tension from one rest period to another, and from one stressful situation to another (b) the relationship between the two muscle areas used—forearm and forehead, and (c) whether, as Malmo found, neurotics respond to instructions to perform tasks by excessive innervation of the musculature.

METHOD

Subjects

A total of 37 neurotic patients was tested. Of this number, 20 were diagnosed as anxious, anxious and depressed, or obsessional, i.e. the “dysthymics” in Eysenck’s terminology (6), (7). The remaining 17 were mixed, mainly hysterical patients, including conversion hysterics, hysterical depressions, hysterical or immature personalities. Many of these hysterical patients also exhibited anxiety symptoms. Analysis of the results was carried out first on the total neurotic group, and then on the sub-groups. (The dysthymic category contained 6 men and 14 women, the hysteric category 9 men and 8 women; average ages were 37.6 and 38.4 years respectively).

The psychotic group contained 18 subjects. These were all schizophrenics, and in a relatively early stage of the illness. (13 men, 5 women, with an average age of 31 years).

The normals ($N = 27$, comprising 11 men, 16 women, with an average age of 35.2 years) were members of the nursing or office staff.

All subjects were given the Taylor Manifest Scale of Anxiety, and Guilford’s S.T.D.C.R. and G.A.N. scales. The latter questionnaires, when

factor analysed, have given rise to two orthogonal factors of neuroticism and introversion-extraversion. It was considered that the application of these scales might supplement the description of personality so briefly provided by psychiatric diagnoses. The questionnaires have already been found useful in discriminating between normals and neurotics, and between hysterics and dysthymics, used as criterion groups for extraversion and introversion respectively (13). It was hoped that they might prove especially helpful in the case of normal subjects, since it was impossible otherwise to gain any information concerning their position on the dimensions of neuroticism and introversion-extraversion.

Instrumentation

Recordings were taken from the frontalis muscle and right extensor muscle. Two single-channel Ediswan electro-physiological amplifiers were used in conjunction with an Ediswan stabilised power unit and pen oscillograph unit. A continuous integrated record (paper speed 1 inch per minute) was obtained from the raw record. Although the integrated record could perhaps have been referred to some average microvoltage, this method would have presented difficulties, and the units which will be quoted are simply log transformations of readings taken at intervals of 15 seconds from the integrated record.

Silver electrodes (cup-type) were employed. In all cases the skin was prepared with methylated spirits, followed by the application of electrode jelly.

Experimental procedure

Recordings were taken from each subject during a programme of alternating rest/stimulus conditions, the whole lasting about one hour. The programme is summarised as follows and amplified in subsequent paragraphs:

1. Relaxation (10-minutes).
2. Instructions.
3. Relaxation (1-minute).
4. Motor frustration task.
5. Relaxation (5-minutes).
6. Instructions.
7. Relaxation (1-minute).
8. Reading—(a) plain, (b) stress.
9. Relaxation (5-minutes).
10. Startle stimulus.

11. Relaxation (10-minutes).
12. Questioning.
13. Completion of questionnaires.

Sainsbury's work pointed to the possibility that psychiatric groups could be distinguished from normals simply in conditions of rest. In the present experiment, therefore, a hospital bed was provided on which the subjects could relax, and a fairly lengthy rest period (10-minutes) allowed initially not only to permit subjects to adapt to the situation, but also to provide data for evaluating rates of relaxation in the different subjects. It was predicted that normals would quickly adapt and readily relax, but that patients would be unable to relax as quickly during the 10-minute period allowed.

In view of the comments made by Malmö (19), and also by Freeman (11) that stress rather than relaxing situations discriminated between normals and patients, it was decided to incorporate several varied stress situations in the experiment. (It is recognised that the word "stress" is used loosely in the immediate context—further reference to this problem will be made later).

A very real complication in the selection of stresses arose from the need to keep the subject in a supine position throughout the duration of the experiment, so as to avoid effects of postural shifts. More than one stress situation was used so that the constancy of response to several disturbances could be ascertained. It was eventually found necessary to suspend recording for short periods in the course of applying the stresses, which were as follows:

(a) the first, introduced after the initial period of relaxation, was a 'motor frustration task'. It was designed on the principles inherent in many so-called frustration tasks, first that the subject's interest should be aroused, and second, he should be shown that he could perform increasingly well, with a minimum of mistakes. After a few trials, he is exhorted to "do his best", but the experimenter throws a switch which, unknown to the subject, renders the simple task more difficult. E.M.G. recording was suspended during the actual performance of the task.

(b) The second stressful situation was based on the delayed feed-back of speech. The subject reads or speaks aloud into a microphone connected to a tape recorder, which records the speech and relays it back to him through earphones .2 seconds later. This apparatus offered a promising source of stimulation, and carried the additional advantage that the subject was able to remain on the hospital bed throughout the reading period.

The general effect of delaying feed-back is described as a retarding of speech, subjects reporting a "stretching out feeling".

It was found necessary to raise the head-end of the bed slightly, so that the passage for reading could be placed before the subject's eyes; short interruption of the recording was necessary while this, and other adjustments, were being made. The period of stress reading was preceded by a short, undisturbed passage of reading.

(c) Startle response. After the relaxation period following reading, a horn was sounded loudly and unexpectedly.

(d) The fourth and last "stress" was adopted from the clinical-experimental type of investigation, in which patients discuss their symptoms while physiological recordings are being taken. Several studies have been reported showing that during such interviews, patients tend to become very physiologically reactive. Control data in these experiments, however, are usually lacking.

A number of traits, selected as having high loadings on Eysenck's neuroticism factor (6) formed the basis of a questionnaire. Each patient was asked to assess himself orally on each trait (e.g. lack of sociability, irritability, depression, etc.) E.M.G. recording was carried out during this questioning period.

Following Malmo's findings that a greater *preparatory* response was evident in the patient groups, a briefly worded instruction preceding the performance of stresses (a) and (b) was given in the present experiment, and the E.M.G. recording continued for one minute following each instruction.

At the conclusion of the experimental session, the Taylor Manifest Scale of Anxiety, in modified version, was given. (Buffer items were not included, also the questions were typed on cards which the subject was requested to sort into appropriate boxes.) The Guilford scales were also completed after E.M.G. recording.

RESULTS

It was noted that the "variability" or fluctuation of the resting record tended to depend upon the level of basal activity. A highly tense muscle would give an integrated record which appeared much more "wavy" than did the more relaxed muscle, from which a very straight, even record was usually obtained. It was decided to deal exclusively with *levels* of tension, rendered independent of variability by log transformations.

It was first ascertained that sex and age were not significantly correlated with resting muscle tension.

Before comparing group means obtained for the resting periods, it was considered necessary to analyse the minute-to-minute trends which might appear, since resting level of tension might be higher (or lower) at the beginning of relaxation periods, and would thus influence the averages obtained for the whole period.

Analysis of trends

Two separate analyses of variance were carried out on the first ten-minute resting period—for forearm and forehead data independently. It was found (a) that the minute-to-minute changes in forehead tension in both normal and neurotic groups were very slight, and the two trends exhibited by the two groups did not differ significantly from each other in extent or direction, and (b) a marked downward trend was evident in the arm tension data throughout the ten minutes, but again this trend is followed closely by both neurotics and normals, and the *F* ratio between them was not significant.

TABLE I
Tension levels * of normals and neurotics during resting sessions

Forehead				
Session No.	Group	Mean	Var.	't'
1	Normal	1.957	.076	1.6 (N.S.)
	Neurotic	2.085	.119	
2	Normal	1.900	.058	1.7 (N.S.)
	Neurotic	2.006	.082	
3	Normal	1.957	.051	1.7 (N.S.)
	Neurotic	2.019	.099	
4	Normal	1.963	.060	2.14 (Signif. at .05 level)
	Neurotic	2.115	.101	
Arm				
1	Normal	1.338	.373	1.77 (N.S.)
	Neurotic	1.639	.538	
2	Normal	1.379	.314	1.58 (N.S.)
	Neurotic	1.648	.299	
3	Normal	1.520	.344	1.67 (N.S.)
	Neurotic	1.821	.688	
4	Normal	1.427	.212	3.00 (Signif. at .01 level)
	Neurotic	1.862	.492	

* Scores are logarithms of integrator readings.

The psychotic group did not co-operate very well in the conditions of relaxation, many small movements occurring which interfered with the production of a good resting record. Analyses of trends in this group were not carried out.

Further analyses revealed no differences in trends between hysterics and anxiety states. It was concluded that no differential rates of relaxation appeared in the data.

Levels of tension—resting conditions

Table I presents data obtained from the normal and complete neurotic group for the resting sessions. It can be seen that, in general, no marked differences are obtained except in the fourth and last resting session, where neurotics are significantly more tense. The latter result can perhaps be explained in terms of a residual (undischarged) covert tension following the successive stress situations; alternatively it may arise from diminishing co-operation and fidgety, restless movements.

TABLE II
Tension levels * of hysterics and dysthymics during resting sessions

Forehead				
Session No.	Group	Mean	Var.	't'
1	Hysterics	1.945	.158	2.35 (Signif beyond .05 level)
	Dysthymics	2.204	.059	
2	Hysterics	1.888	.090	2.43 (Signif at .02 level)
	Dysthymics	2.107	.059	
3	Hysterics	1.918	.074	4.39 (Signif at .01 level)
	Dysthymics	2.188	.061	
4	Hysterics	1.935	.094	3.51 (Signif beyond .02 level)
	Dysthymics	2.268	.058	
Arm				
1	Hysterics	1.380	.358	2.09 (Signif. at .05 level)
	Dysthymics	1.860	.607	
2	Hysterics	1.435	.610	1.531 (N.S.)
	Dysthymics	1.833	.622	
3	Hysterics	1.482	.549	2.46 (Signif. at .02 level)
	Dysthymics	2.110	.653	
4	Hysterics	1.600	.351	2.25 (Signif. beyond .05 level)
	Dysthymics	2.085	.521	

* Scores are logarithms of integrator readings.

The analysis of resting conditions was then repeated on the two sub-groups—dysthymics and hysterics; results are presented in Table II. In spite of the uncertainty attaching to many of the psychiatric diagnoses, there are clear and consistent differences in levels of muscular tension between these two sub-groups. Dysthymics as a group appear to be more tense, muscularly, than hysterics.

From the figures given it can be observed that normals tend to fall between the dysthymics and hysterics in level of forehead tension, although they are closer to the hysteric group than to the dysthymic. The two neurotic sub-groups tend to have higher arm tension scores than the normals.

Most of the psychotic group exhibited fairly acute symptoms, and, as indicated earlier, testing was frequently carried out only with difficulty. Co-operation was poor throughout the whole session; in spite of repeated requests, these patients were unable to remain still for any length of time. The means and significance levels obtained from a comparison of psychotic

TABLE III
Tension levels * of psychotics and normals during resting sessions

<i>Forehead</i>			
Session No.	Group	Mean	't'
1	Psychotics	2.148	2.1 (Signif. at .05 level)
	Normals	1.957	
2	Psychotics	2.112	2.65 (Signif. beyond .02 level)
	Normals	1.900	
3	Psychotics	2.146	2.36 (Signif. beyond .05 level)
	Normals	1.957	
4	Psychotics	2.189	3.20 (Signif. beyond .01 level)
	Normals	1.963	
<i>Arms</i>			
1	Psychotics	1.618	1.55 (N.S.)
	Normals	1.338	
2	Psychotics	1.722	1.90 (N.S.)
	Normals	1.379	
3	Psychotics	2.055	3.15 (Signif. beyond .01 level)
	Normals	1.520	
4	Psychotics	1.961	3.34 (Signif. beyond .01 level)
	Normals	1.427	

* Scores are logarithms of integrator readings.

and normal data are given in Table III, which shows that in most resting sessions, psychotics are significantly more tense than normals. It should be made clear, however, that it was virtually impossible to eliminate small movements in the psychotic group; the contribution of this "interference" to the tension level recorded might well have been considerable. In the absence of records of actual overt movement, it is difficult to make precise statements concerning this point.

Levels of tension—stress conditions

The primary classification of normal/neurotic was retained for the first part of the analysis of behaviour under stress conditions.¹ While all the instruction minute averages show neurotics to be more tense, only one score (forehead tension during instruction period 2) reaches the 5% level of significance. Since hysteric and normal means for this minute are very similar, the level of significance is probably a function of the high level reached by the dysthymics.

Questioning (stress situation 4) gives rise to marked differences in tension, level of arm tension in neurotics becoming very significantly greater, while forehead tension in neurotics is not quite significantly greater than normals. It is interesting that this difference in arm tension during questioning is contributed not only by dysthymics but also by hysteric. This suggests that the imposed stress differentially affected individuals with respect to their normal/neurotic classification.

While the subjects were answering the questions, a considerable amount of gestural movement was noted; again, it is possible that these gestures contributed to the high level of muscle activity recorded.

Apart from the instances discussed, the stress situations did not discriminate between normals and neurotics.

Analysis of the hysteric/dysthymic stress data revealed only three occasions on which the two groups differed significantly: forehead tension during instructions 1 and 2, and arm tension during instruction 2. Since,

¹ While the present work was being carried out, a publication entitled 'E.M.G. reaction to strong auditory stimulation in psychiatric patients' appeared, describing work which had been carried out by Davis *et al.* (1). These authors found it necessary to analyse muscle responses following a startle stimulus in periods of 1/10th seconds, and observed that the period beginning after .2 second (called the after-response period) was that in which patients and controls differed in reactivity. The integrator used in our study did not permit readings to be taken at such brief time intervals as these, and it was not felt that Davis' careful analysis could be repeated with our data. Immediate muscular reaction to the startle stimulus was not, therefore, analysed in the present study.

however, instruction minute average scores, and not *change* scores, were obtained, the strong possibility that degree of response to instructions depended upon preceding level of tension must be considered here. A close examination of the minute-to-minute scores appears to reveal a genuine difference in arm responsiveness to instructions (dysthymics manifesting greater reactivity) but obviously a more careful analysis of the data using valid "change" measures is required. So far, little work seems to have been accomplished in electromyography concerning the relationship of stimulus-induced changes to basal activity, although Davis (3) has suggested "a tendency for responses to be proportional to the base level of muscle action potentials".

Tension measures during the period of stress reading did not discriminate between psychotics and normals. Instruction and questioning scores were not assessed for psychotics in view of their gross movement responses to these stimuli.

On the whole, little discrimination between groups is achieved from the analysis of the stress data. The most outstanding exception is arm tension difference between normals and neurotics while responding verbally to items concerning symptoms.

Reliability of measurement

Estimates of consistency were derived from the formula $\frac{S_1^2 - S_2^2}{S_1^2 + S_2^2}$ where S_1^2 is the mean square variance between persons, and S_2^2 is the mean square error variance. Average scores were obtained, for each subject, from four resting sessions, and five stress conditions. Separate analyses were carried out on forehead and arm scores. The results of the twelve analyses of variance (comprising arm: rest and stress; forehead: rest and stress, for the three groups—normals, hysterics and dysthymics) are given in Table IV. In spite of the varied interruptions during the

TABLE IV
Consistency * of tension scores

		Normals (N = 27)	Hysterics (N = 17)	Dysthymics (N = 20)
Forehead	Rest	.892	.768	.636
Forehead	Stress	.715	.609	.649
Arm	Rest	.697	.848	.816
Arm	Stress	.520	.695	.667

* Estimates obtained from analyses of variance—see text for further information.

experimental session, the levels of tension maintained during the resting periods are strikingly similar. The variability of individuals' mean scores from one stress situation to another is, as would be expected, larger than for the resting situations. Nevertheless, quite high consistency of measurement between stresses is apparent.

Data were also obtained from a small group of patients ($N = 7$) who were tested for a period of 10-minutes at the same time on four consecutive days. Analyses of variance were carried out on the results, one for arm and one for forehead tension. The variance between days was not significant, i.e. there was no general decrease or increase in tension during the four days. Estimates of reliability gave a coefficient of .762 for forehead tension, and .826 for arm tension. These are, of course, highly significant.

Further analyses of forehead tension during rest and stress were carried out. Each person's average resting score, i.e. the mean of his four resting session scores, was compared with a similar score based on the five "stress" situations. Estimates of consistency were obtained as before; the resulting coefficients were: .849 for normals, .561 for dysthymics, and .585 for hysterics. Thus a consistent level of tension is exhibited during several different conditions.

In general, very low correlations were obtained between arm and forehead tension scores during the resting sessions. There was a tendency, however, for hysterics to have lower (almost zero) correlations than the dysthymics. Coefficients in the latter group averaged .4, bordering on the 5% level of significance.

Questionnaire data

It will be remembered that Taylor's Manifest Anxiety Scale, and Guilford's S.T.D.C.R. and G.A.N. scales were administered after the recording programme.

Normals are very significantly differentiated from neurotics on the three best neuroticism measures, namely the Anxiety Scale, 'D', and 'N'.

The introversion-extraversion scales ('G' and 'R') do not differentiate anxiety states from hysterics in this particular experiment, and thus do not support earlier findings (Hildebrand (13), Franks (9)). They do, however, correlate together well (neurotics, $N = 37$, $r = .60$) as would be expected. Similarly, the Anxiety Scale and 'N' intercorrelate to the extent of $-.638$ (neurotics, $n = 37$, high scores on the Anxiety scale and low scores on 'N' indicating neuroticism).

It can be seen, therefore, that the scales themselves behave as would

have been predicted on the basis of earlier experimental data. They have been used extensively at this Institute with both patients and normals (Hildebrand (13), George (12), Eysenck (8), and Franks (9)) so that comparison of the present means with others is possible.

The normal mean on the 'R' scale obtained here (33.8, S.D. 12) compared closely with those from large samples obtained by George ($N = 95$, mean 35.6, S.D. 12, and $N = 500$, mean 36.3, S.D. 10) and Eysenck (200 males, mean 37.13, S.D. 11.9; 200 females, mean 37.84, S.D. 11), although biased a little towards the introvert side. The means of the neurotic sub-groups differ fairly considerably from earlier figures, although there is close agreement between Hildebrand's mean for anxiety states—27, and the present dysthymic group—28.2. The present hysteric scores, however, manifest a very gross departure from previous averages, the mean being very much lower than those obtained before. It is probable that some explanation of these discrepant 'R' scale results and the failure of this scale to differentiate between hysterics and dysthymics can be found in the extremely heterogeneous nature of the hysteric group used in this study. In many of these patients, anxiety symptoms were very dominant. The dysthymics, on the other hand, were more carefully selected, and represented a more homogeneous group.

DISCUSSION

Several points of theoretical and practical interest arise from the results detailed in the foregoing paragraphs. Taking the questionnaire results first, it seems fair to conclude that the hysteric group was somewhat atypical. Dysthymics and hysterics were similar not only in degree of neuroticism, as would be expected, but also in introversion-extraversion measured by the 'R' scale, whereas earlier work had shown hysterics to be significantly more extraverted. The discrepancy may well arise from the prevalence of anxiety symptoms in the present hysteric group.

However, differences in levels of muscle tension between dysthymics and hysterics are very apparent, the high tension of the former group being a persistent feature during several rest situations. (It is not thought that the records obtained from these subjects were distorted by fidgety, restless movements, as appeared to be the case with psychotic patients). Further experimentation, with fresh groups of hysterics and dysthymics is needed to provide confirmation of these findings.

The second important point is that, on the whole, "stress" conditions do not discriminate well between patients and normals in the present work, although some support is adduced for Malmo's finding that patients

tend to respond to preparatory instructions with greater tension than normal. A marked exception to the lack of discrimination is the strong reaction to questioning evidenced by the neurotic group.

It would be premature at the present stage to conclude (as have some authors) that all patients over-react to all everyday situations, and future research should probably be directed towards ascertaining those conditions under which discrimination between groups is achieved. A more careful analysis of our use of the term "stress" is undoubtedly necessary. Lazarus (17) has suggested that "stress" is best regarded as an intervening variable, since it "cannot be defined in terms of stimulus or response operations alone". Although some progress has been made towards establishing individual differences in response patterns, the need to analyse the stimulus situations employed in psychological experiments is becoming increasingly felt. Only then may the usefulness of other concepts also employed as intervening variables (e.g. "emotion", "effort", "motivation") be experimentally established and differentiated.

Investigations into response patterns have been carried out by Lacey, using autonomic measures (15, 16) and Malmo (autonomic and skeletal functions, 20, 21) who have both found it necessary to employ allied concepts of "response" and "symptom" specificity. Patterning within the musculature (as within the autonomic nervous system) has been noted; earlier (21) Malmo had suggested that "there seemed to be a general factor for muscular tension in the individual", particularly during "active" performance, but this has been modified (23). An interesting point is that these authors' findings lead them to believe that such response patterns occur whatever the stress, although there are individual differences in stability of response. Results of the present study, too, tend to support such a conclusion, since fairly high individual consistency appears in levels of tension from one situation to another. There is also a slight tendency for dysthymics to be more generally tense than hysterics, although the data obtained here are too meagre to permit of any firm suggestions.

Davis, however, in a very careful study using normal subjects (13) has concluded that qualitatively different stimuli (e.g. cutaneous, auditory) produce somewhat different response patterns. He concluded that "in general, there is evidence of a number of different but overlapping response systems whose differential action is related to the nature of the stimulus and whose characteristics can be modified by stimulus repetition". The questioning type of situation employed in the present study would seem to be of a more complex "ideational" kind, where meaning and attitude may be of importance, different from the brief sensory stimulation provided

by Davis. A more molecular analysis may be found necessary in the latter type of situation, since physiological events such as latency and adaptation are intimately involved, particularly with repetition of the stimulus. Complex stimuli of an "ideational" or more broad situational type probably have more ramifying effects, persisting in certain cases over a much longer period of time, and perhaps involving re-exciting feed-back systems.

It is important not only to speculate but to experiment upon the pattern of physiological responses to such apparently varied stimuli, and to examine the usefulness of such different stimuli in producing discrepancies in normal/neurotic reactions. While the present finding of differences between normals and neurotics during questioning may support the view that normal/patient differences become marked only when areas of 'low frustration tolerance' are touched upon, it will be recalled that Davis (1) found delayed and very brief increase of tension in patients following a simple auditory stimulus.

Tensional responses to many kind of disturbing situations are frequently believed to be concomitants of states such as "anxiety". As indicated earlier, there is insufficient experimental evidence available to conclude that the demonstrably high tension level in certain patients does in fact arise from "anxiety", although both Malmo and Sainsbury have expressed this belief. Davis (3) refrains from using terms like fear or anxiety to label his response patterns; more cautiously he expressed the hope by his analyses to "establish some landmarks for plotting the terrain". There is, of course, considerable experimental evidence to show that marked tensional increase is apparent in subjects while simply listening (for example, to a short story, Wallerstein (27)), being possibly related to attention or effort. Malmo (23) himself comments: "it is clear how artificial it is to reserve physiological concepts for 'emotional disturbance', excluding them from the sphere of 'cognitive acts' Finding an explanation which will account for cognitive as well as affective aspects of tensional change is . . . one of the things which is required in order to advance theory in this field".

It becomes clear that additional experimental data are required in order that present and earlier evidence may receive adequate evaluation. A more rigorous and critical inspection of the use of "anxiety" concepts is warranted, and theories of emotion expanded (or constricted) so as to rest firmly upon the rapidly-growing field of psycho-physiological research.

SUMMARY

Levels of muscle tension of the forehead and right forearm were assessed electromyographically for psychotics, neurotics, and normals during a variety of rest/stimulus conditions. No differential *trends* of relaxation were noted.

In general, resting tension did not reliably differentiate neurotics from normals; dysthymics, however, were found to be significantly more tense than were hysterics. Psychotics were significantly more tense than normal in a resting situation.

Stress situations do not differentiate any of the groups except in one instance: when responding to questions concerning their symptoms, neurotics as a group manifest a very significantly greater level of arm tension.

Tension scores are shown to be highly consistent, even over a period of days.

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THE PERCEPTION OF INCONGRUITY BY YOUNG CHILDREN

BY

ISAAC BEHAR AND WILLIAM BEVAN

Emory University

An appreciation of how persons come to comprehend the presence of incongruities is prerequisite to the psychologist's understanding of the rational quality of mature human thought. For, perhaps, it is this capacity more than anything else, that spells the difference between the disciplined analysis of adult thinking and the autistic compounding of ideas that characterizes that of children. Intimately tied up with this is the problem of perceptual development. Common sense suggests that if an individual is to handle discriminatively the incongruities that confront him, he must be aware of their existence. To say only that perception is a necessary antecedent to conceptualization is, however, to be overly simple. No sharp line can be drawn between a perception and a well-tested construct, for perceptions depend as much upon an individual's expectations concerning a stimulus configuration as upon the configuration's properties *per se*. The structure of expectations can only have reference to significances acquired by the configuration in the perceiver's previous intercourse with it.

Dixon (1) has shown that when children understand the meaning of the concepts *big* and *little* they are also sensitive to the contradiction produced by the ring-segments illusion when the segments, different in color, are interchanged in position. The present experiment was designed to trace the appearance of an awareness of incongruity in young children resulting from the nature of the assumption of form associated with a class of commonly-experienced objects, and, further, to suggest the importance of the pictorial clue or clues related to this assumption (expectation) for the impression of incongruity.

EXPERIMENTAL SITUATION

Subjects

Subjects were 74 children, from four years, two months to seven years, eleven months of age, drawn from a private nursery school, a private grammar school and a public grammar school. The mean IQ's, based on performance on the Ammons Full-Range Picture Vocabulary Test, of the

several age groups were as follows: four years, 136.8 ($N = 12$); five years, 123.9 ($N = 20$); six years, 121.9 ($N = 28$); and seven years, 120.9 ($N = 14$).

Stimulus material

Sensitivity to perceptual incongruity was measured by rating the childrens' responses to a photograph of a three-sided building of the "Flat-Iron" type, $3\frac{1}{2} \times 4\frac{1}{2}$ inches, mounted on a stiff cardboard mat. The photograph had been taken so that only the front of the building was visible, yielding the impression of a two-dimensional facade (Fig. 1). It was not retouched.

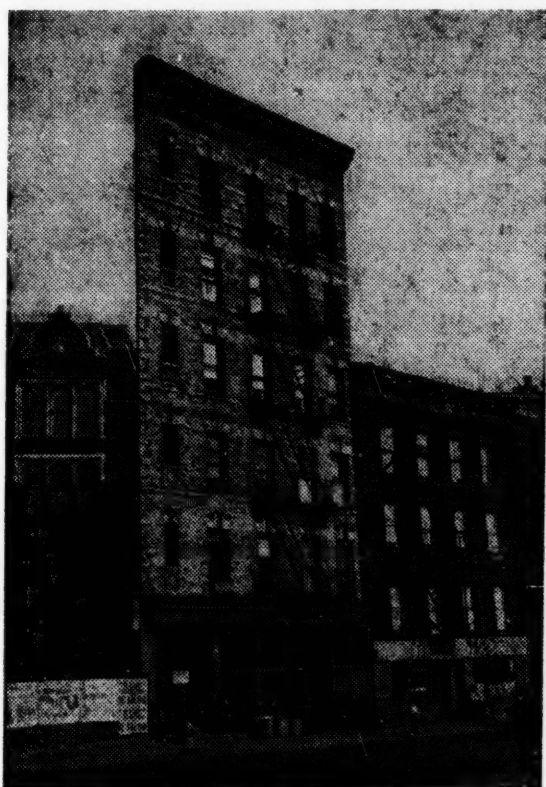


Fig. 1. Photograph of a three-sided building used as the stimulus figure.

Measures of intelligence

Each child was given two tests from which estimates of *MA* and *IQ* were derived: The Ammons Full-Range Picture Vocabulary Test, a measure of verbal comprehension, and the Hildreth Animal Puzzle Test, a measure of mental capacity in a jig-saw type puzzle-solving situation. The upper limit of the last mentioned test was $MA = 8$, a value exceeded by 17 subjects, mostly in the two upper age groups. Mental ages obtained with the two tests intercorrelate .491 ($N = 57$).

Test Procedure

Each child was observed individually for about 20–30 minutes in a play setting. The children looked forward to this session, and frequently asked to be allowed to come back for more play at another time. The subject was seated at a low table and, after rapport had been established, shown the photograph and asked, "Tell me what you see in the picture." If awareness of the incongruity was not verbalized, he was asked, "Is there anything funny about (odd about, unusual about) the picture?" If this did not elicit recognition, the experimenter ran his finger across the vertical dimension of the picture, over the incongruous building, and asked, "Is there anything funny about (unusual about, etc.) this building?" If he still was unsuccessful, he asked the child a series of questions: "Is this building a regular or a funny building?" "Does it have a back?" "If so, where is the back?" "If it is missing, where should it be?" Finally, if interrogation yielded any impression of recognition, the child was asked, "Why does the building look like it does?" and, "How did it get that way?" All of the child's verbal responses, gestures, facial expressions, etc. were recorded.

In order to maintain the atmosphere of play, the child was introduced to the Hildreth Puzzles by being asked if he liked animals and then to the Ammons by being asked if he would like to play a game called "Show Me."

Measurement of sensitivity to incongruity

Three judges, one instructor and two advanced graduate students in psychology, independently evaluated the protocol for each child with the following rating scale:

0. No evidence of any recognition of incongruity after extensive interrogation.
1. Vaguely expressed recognition only after being asked where the back of the building was.
2. Recognition expressed only after being asked where the back of the building was.
3. Clearly expressed recognition only after being asked where the back of the building was.
4. Vaguely expressed recognition only after being asked if there was anything odd about the building.
5. Recognition expressed only after being asked if there was anything odd about the building.

6. Clearly expressed recognition only after being asked if there was anything odd about the building.
7. Vaguely expressed recognition only after being asked if there was anything odd about the picture.
8. Recognition expressed only after being asked if there was anything odd about the picture.
9. Clearly expressed recognition only after being asked if there was anything odd about the picture.
10. Spontaneous recognition of incongruity, vaguely expressed.
11. Spontaneous recognition of incongruity, expressed.
12. Spontaneous recognition of incongruity, clearly expressed.

Product-moment correlation coefficients between each rater and every other rater were in all cases .986. Ratings were therefore averaged for further treatment.

Summary of explanations of incongruity

Responses to the final questions, "Why does the building look like it does?" "How did it get that way?" etc., given by children, to whom at least two judges had assigned a rating of one or greater, were surveyed to get some notion of how the incongruity was explained. This survey yielded the following nine-category system for classifying responses:

1. No explanation (e.g., "I don't know how it could have happened.").
2. It was built that way, but to serve a purpose (e.g., "They built it that way so that people could live there.").
3. It was built incorrectly (e.g., "They made a mistake when they built it.").
4. It was produced by an act of Nature—fire, storm, accident, etc. (e.g., "It all burned down but this part.").
5. It was produced by a deliberate human act (e.g., "They're tearing it down.").
6. Construction is incomplete (e.g., "They haven't finished making it yet.").
7. It is a facade—for staging, etc. (e.g., "This one is for decoration, they live in the other ones.").
8. It is poorly represented or purposefully misrepresented (e.g., "They made the picture so that the back didn't show.").
9. It is a triangular building.

The relative frequency of responses assigned to each category by the several age groups was then tabulated.

RESULTS

Age and sensitivity to the perception of incongruity

Table I presents Pearson product-moment correlation coefficients between rating of response to the picture and chronological age, mental age, and *IQ*.

TABLE I

The relationship between ratings of perceptual incongruity and chronological age, mental age and *IQ*

Comparison	<i>N</i>	<i>r</i>	<i>P</i>
Rating × Chron. Age.	74	.46	.01
Rating × M.A. (Ammons).	74	.42	.01
Rating × M.A. (Hildreth).	57	.30	.05
Rating × <i>IQ</i> (Ammons).	74	— .04	N.S.
Rating × <i>IQ</i> (Hildreth).	57	— .26	.05

Inspection reveals one obvious fact: Capacity to recognize the perceptual incongruity presented in this study depends on level of development rather than on relative brightness. While greater success comes with both greater chronological and mental age, the correlation between the rating of sensitivity and Ammon's *IQ* is essentially zero.

The negative correlation between rating and Hildreth *IQ*, barely significant at the .05 level, does not change this conclusion, for it may be reasonably explained as an artifact of sampling and the limits of the test. The four year olds, drawn from a private nursery, were children of professors, doctors, and other professional people, and were, without exception, highly superior intellectually. In contrast the spread of *IQ*'s for the six and seven year olds ranged from below 100 and, since the

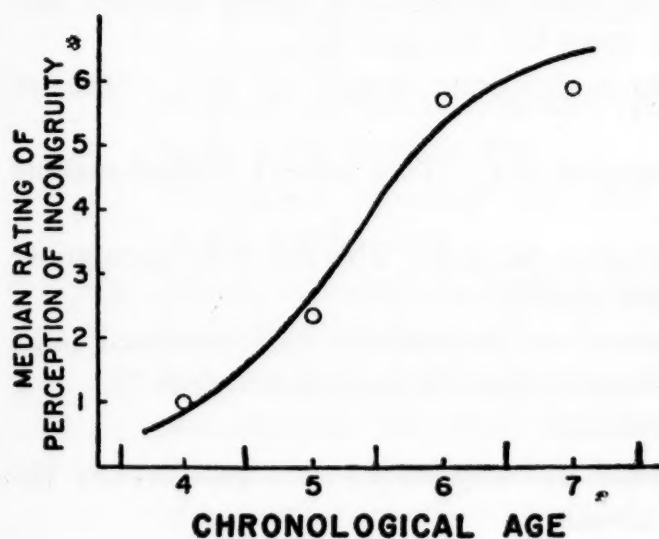


Fig. 2. Relationship between chronological age and median rating of perception of incongruity. The digits on the abscissa mark the midpoint of each interval.

test can be scored to only $MA = 8$, the brighter 40% of the two upper age groups were eliminated from our calculations.

Figure 2 portrays the relationship between chronological age and median rating. The curve, graphically fitted through four points, is the ogive commonly encountered in developmental functions. It suggests that there is little or no evidence of recognition below four, and that development is most rapid between $5\frac{1}{2}$ and $6\frac{1}{2}$, after which it progresses much more slowly. If a critical age is to be set for the appearance of incongruity it is probably about $6\frac{1}{2}$ years; for in the age group 6 to 7, in excess of half of the cases received rating of 6 or greater.

Explanations of incongruity

Table II reports the relative frequency with which the various types of explanation were offered by each age group.

TABLE II
Relative frequency of the several explanations of perceptual incongruity offered by children in different age groups

Age	Number of responses	Category								
		1	2	3	4	5	6	7	8	9
4	8	.13	.37	.25		.25				
5	33	.03	.06	.15	.40	.12		.09	.12	.03
6	31	.06	.03	.13	.26	.16	.10	.03	.20	.03
7	22	.15		.20	.40	.20				.05

Some of the youngest children recognized the apparently two-dimensional nature of the center building, but for the most part (37%) apparently did not regard it to be unusual or outside the category, "regular building." The remaining three age groups most frequently rationalized its odd appearance as the result of an act of Nature; it was an ordinary building that had all but its forward wall destroyed by a fire, a storm, an explosion, etc.

DISCUSSION

The capacity for responding to the stimulus used in this study as incongruous appears to depend upon level of development rather than relative brightness. One possible way of accounting for this is in terms of the child's having acquired the verbal skills necessary for communication. We regard this, however, to be an overly simple explanation, for the flexibility of our interviewing technique plus the record of facial expressions and spontaneous remarks prompt us to think of it more as a matter of perceptual organization than of functional vocabulary. More specifically, it would appear to relate to the child's ability to respond to

the representation of the three-dimensional world in two-dimensional space. Support for this is suggested by the fact that the cues of linear perspective and relative size are not clearly utilized in children's drawings until six, seven, or older (3). Certainly, as Updegraph has reported (4), four year olds who are very sophisticated in spatial orientation, cannot interpret the representation of space in terms of perspective, relative size, shadows, etc. That this involves more than the capacity for spatial orientation is further brought out by Gesell's *et al.* description of the three year olds' skill at localizing objects (2). Finally, the present data stress the importance of experience to conceptualization. Only three of the children gave correct explanations of the incongruity. Two of these three indicated having seen such buildings with only three sides. Adults to whom the picture was shown, in contrast, gave immediate indication of awareness of the incongruity but most frequently explained it by identifying the building as a facade (stage set, etc.) or suspecting some trick of photography or photographic reproduction. If it is possible to talk of a critical age for being aware of the incongruity, it is probably defined by skill in using cues in the representation of space. Prolificness of explanation appears to be related to the richness of the child's autistic thinking. This latter function is suggested by the larger number of categories used by the five and six year olds, compared with the four's and seven's.

SUMMARY

Seventy-four children, four to seven years of age, were asked to describe what they saw in a photograph of a three-sided building taken so that it appeared, in contrast to the buildings surrounding it, to have only one wall, although at the same time it appeared to be, or to have been, inhabited. The readiness with which the awareness of this incongruity was expressed was found to be a positive monotonic function of both mental and chronological age. The most frequent rationalization offered for the incongruity was that the building had been damaged by some act of Nature. Only three children, two of whom had seen such buildings, offered the correct interpretation.

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"WARM-UP" IN PURSUIT ROTOR LEARNING AS A FUNCTION OF THE EXTINCTION OF CONDITIONED INHIBITION

BY

H. J. EYSENCK

Institute of Psychiatry (University of London), Maudsley Hospital (1)

1. INTRODUCTION

It is a well-known principle in science that measurement is possible and meaningful only in terms of a theory, or set of theories. In psychology there has been a curious bifurcation. Psychometrists have perfected the technique of psychological *measurement* to a considerable degree, but have neglected very largely the whole area of psychological *theory*. Experimental psychologists, particularly those in the field of learning theory, have been very active in the development and testing of *theories* but have tended to neglect the task of *measurement*. In doing so they have tried, as far as possible, to suppress the major source of variance in their data, namely, that concerned with individual differences. Only occasionally, as in the case of a littleknown paper by Hull (2), are individual differences permitted to emerge, but only as modifying constants in the learning equations. The writer has attempted in a series of papers (3, 4, 5, 6) to integrate learning and perceptual theory, on the one hand, and the field of individual differences and personality dimensions on the other. In testing some of the predictions made, it became apparent that certain of the theories basic to such measurement were, in fact, incorrect, and a number of experiments had to be carried out in order to settle the theoretical issues raised. The present paper is concerned with the description of some of these issues, and a brief account of the experiments conducted and results achieved.

This difficulty arose particularly with reference to the prediction made by the writer in terms of his general theory of extraversion-introversion (7) that *reminiscence effects would be stronger for extraverts than for introverts*. The experimental testing of this prediction depends very much on the existence of a quantitative measure of reminiscence, and this in turn can only be derived from a more general theory of this phenomenon. Such a theory exists (8, 9) but its direct application to our problem is prevented by effects, such as "warm-up", which do not form part of the theory. It became necessary, therefore, to investigate the theoretical import of these

additional phenomena in order to make possible the testing of our primary hypothesis. This investigation is reported in the present paper; with the knowledge gained in this set of experiments it became possible to submit our primary hypothesis to a proper experimental test which verified it at a high level of statistical significance (10). It is to be expected that similar difficulties will arise in relation to the measurement of other variables which play a part in the theories of experimental psychologists and which appear also to be relevant to psychologists interested in individual differences and the "structure of personality".

2. THE THEORY OF "WARM-UP" DECREMENT

The theoretical and experimental analysis of the curve of work was begun by Kraepelin (11) and his students around the turn of the century. Thorndike's (12) criticisms of these early concepts helped to clarify the situation. In more recent years, interest has shifted somewhat to the effect of rest pauses and to the experimental study of phenomena such as the so-called "warm-up" effect. This is defined as a sudden initial rise in performance after a rest, which is succeeded by more nearly level stretches of the work curve, or even by stretches showing a downward trend. The theoretical basis for this phenomenon is well conveyed in the term used; practice is supposed to be facilitated by mental attitudes, muscular postures, and the like, which are lost during rest, and which have to be reinstated during the first few seconds of practice before optimum performance is possible.

Learning theorists do not appear to have given much thought to the importance of the influence of warm-up effects on the curve of learning, although the work of Bell (13), and particularly Ammons (14) forced it upon their notice. Ammons specifically introduced the concept of D_{wu} , or warm-up decrement, into his system. It is defined in terms of the relationships shown in Figure 1, which is quoted from his work: - "At any trial D_{wu} will be the vertical difference between line B and the postrest performance curve where line B is higher. D_{wu} is thus essentially the inverse of Bell's idea of "warming-up"." The Bell-Ammons concept of warm-up decrement appears to be a necessary complement to the Hullian treatment of learning, which does not attempt to account for the sudden continued rise in performance after a rest period.

In our own work, the concept of warm-up decrement became of importance only because of its relevance to the measurement of another hypothetical construct, namely, that of reactive inhibition (15). In the Hullian theory, reactive inhibition is accumulated during massed practice and dissipates during a succeeding rest. If the rest is long enough to allow of complete

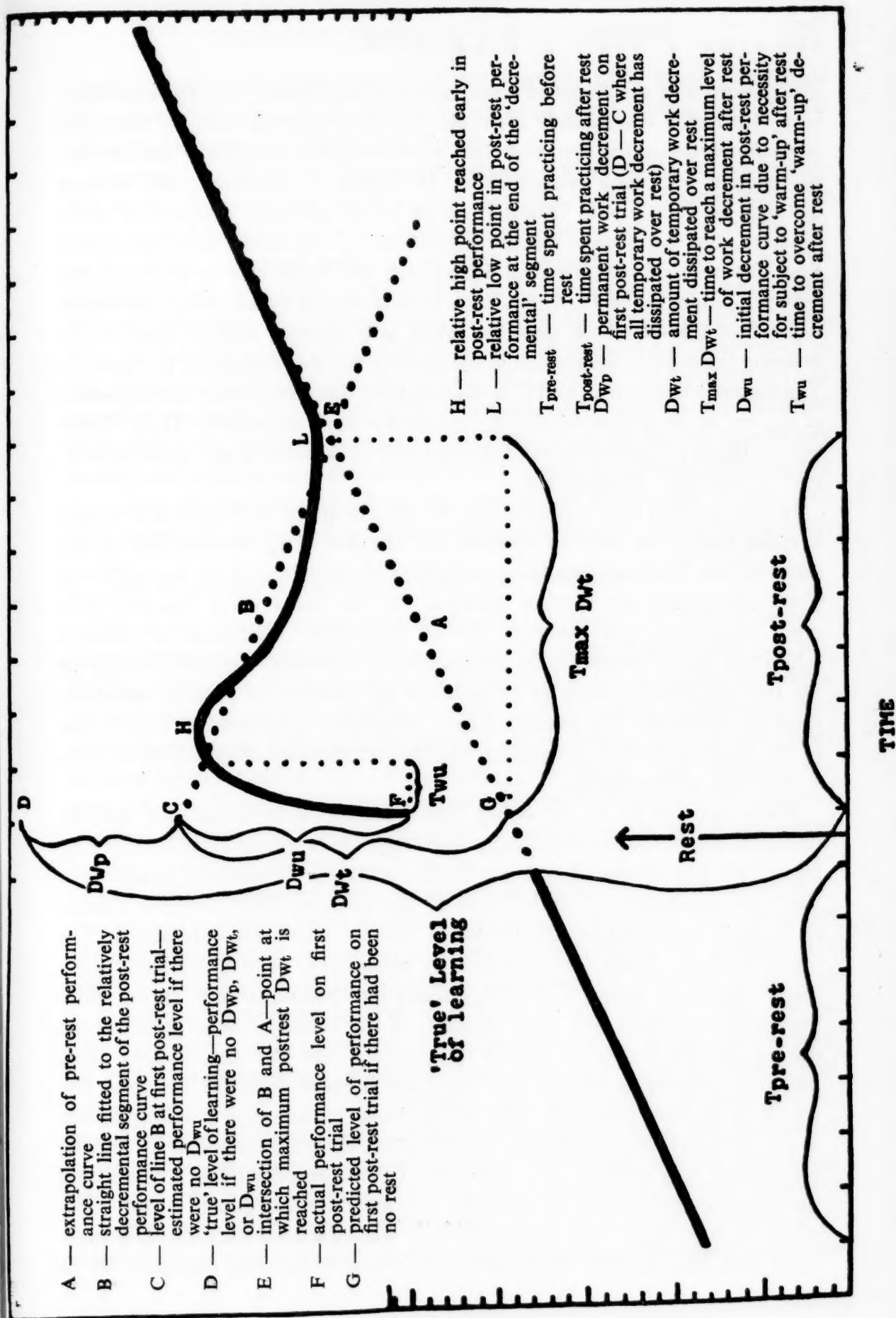


Fig. 1. Diagrammatic representation of Ammons' theory of warm-up effect. Quoted by permission of the *Psychological Review*.

dissipation, then the so-called reminiscence phenomenon, i.e., the increment in performance *immediately after* the rest as compared with performance *immediately before* the rest, will serve as an adequate measure of the amount of reactive inhibition accumulated. In Figure 1, therefore, the vertical distance between points G and F would be an adequate measure of reminiscence, and therefore of reactive inhibition (I_R). Ammons, however, argues that this way of measurement leaves out the rapid rise in performance between points F and H, which in his view is caused by warm-up. He would insist that this warm-up must be taken into account and he does so by defining the point C very much in the manner shown graphically in Figure 1. The distance between G and C is his estimate of reminiscence, and therefore of I_R , or, as he prefers to call it, temporary work decrement (D_{wt}). These two methods of measuring reminiscence will be referred to as "uncorrected" and "corrected" respectively.

It will be seen that a decision as to the adequacy of Ammons's theory is quite vital if we wish to measure the amount of I_R accumulated at the point of rest. Not only would our estimates of reminiscence be very different if we substituted the vertical distance CG for the vertical distance FG; more important, the measurement of individual differences in reactive inhibition would become quite impossible, as individual performance curves are too irregular to allow us to estimate the position of point C with any kind of precision. The present paper, therefore, is concerned with a test of the hypothesis that the sudden rise in performance after rest is, in fact, a warm-up phenomenon.

There appears to be no doubt that warm-up does occur and that its influence can be manipulated experimentally. On nonsense syllable learning Irion (16) has shown that there is less warm-up if subjects retain their "set" by continuing to sit in front of the memory drum without change of posture, pronouncing the names of colours which are exhibited in the memory drum. Similar experiments by others (17, 18, 19, 20) have given results much in support of this general conclusion, and it seems quite impossible to doubt that part, at least, of the sudden rise in performance during the first few trials after a rest pause is due to warm-up (21). It is, however, difficult to believe that warm-up is responsible for the total rise which is, in fact, observed. One of the main reasons for thinking so lies in the time intervals concerned. In Ammons's diagram the rise from F to H takes place very quickly, i.e., within one or two practice trials. Considering the hypothesized nature of warm-up, i.e., the "shaking down" of the organism, both mentally and physically, into well-practiced attitudes and muscular sets, that is quite reasonable; we would expect the organism to be

"back on the job" within ten or twenty seconds. (The usual single trial lasts about ten seconds in pursuit rotor work from which most of the evidence on warm-up effects has come).

In actual fact, however, Ammons's diagram is misleading; the time intervals involved are usually much longer. The three curves appearing in the lower part of Figure 2 record an experiment on the pursuit rotor, conducted by the writer, in which three sets of thirty consecutive 10-second periods of practice are separated by two 10-minute rest pauses. (The records of 50 male University students are averaged in this figure) (22). It will be seen that the H-point in the curves following the rest pause is not reached until 110 seconds and 60 seconds respectively have elapsed. These times are far too long to make "warm-up" a likely hypothetical cause for the observed phenomena. It becomes necessary, therefore, to look for an alternative theory, and then to make deductions from this new hypothesis which would contradict deductions made from the "warm-up" hypothesis. In this way only would we be enabled to decide on the adequacy of Ammons's theory.

3. AN ALTERNATIVE THEORY OF POST-REST INCREMENT

The theory here proposed follows directly from Hull's postulates and the experimental and theoretical extension of that work made by Kimble (23, 24). Briefly stated, this theory treats I_R as a negative drive. I_R builds up during massed practice until it reaches a point where a brief involuntary rest pause is enforced. (This concept will be abbreviated I.R.P. in this paper to save space). During this I.R.P. some I_R dissipates, thus lowering the amount of inhibition present sufficiently to make resumption of practice possible. Practice then continues until I_R again reaches the point where another I.R.P. is enforced, and so on.

As I_R , being a negative drive, is reduced during these hypothetical I.R.P.'s, these act as a reinforcement for the prevailing state of affairs. The prevailing state of affairs being one of *not reacting*, we thus obtain the concept of a habit of not reacting which becomes conditioned the moment I_R reaches a sufficiently high concentration to enforce the rest periods which act as reinforcements. (This concentration will be referred to as the *critical level* of I_R .) This conditioned inhibition (${}_sI_R$) does not dissipate during rest because it is a habit; it has therefore been symbolized by Ammons as *permanent* work decrement (D_{wp}). As Kimble has indicated, this permanent work decrement can be shown to exist and can be measured by comparing scores of a group of subjects who have done work in conditions of massed practice, followed by rest, with the scores made by a group of people who have done an equal amount of distributed practice. (The assumption here

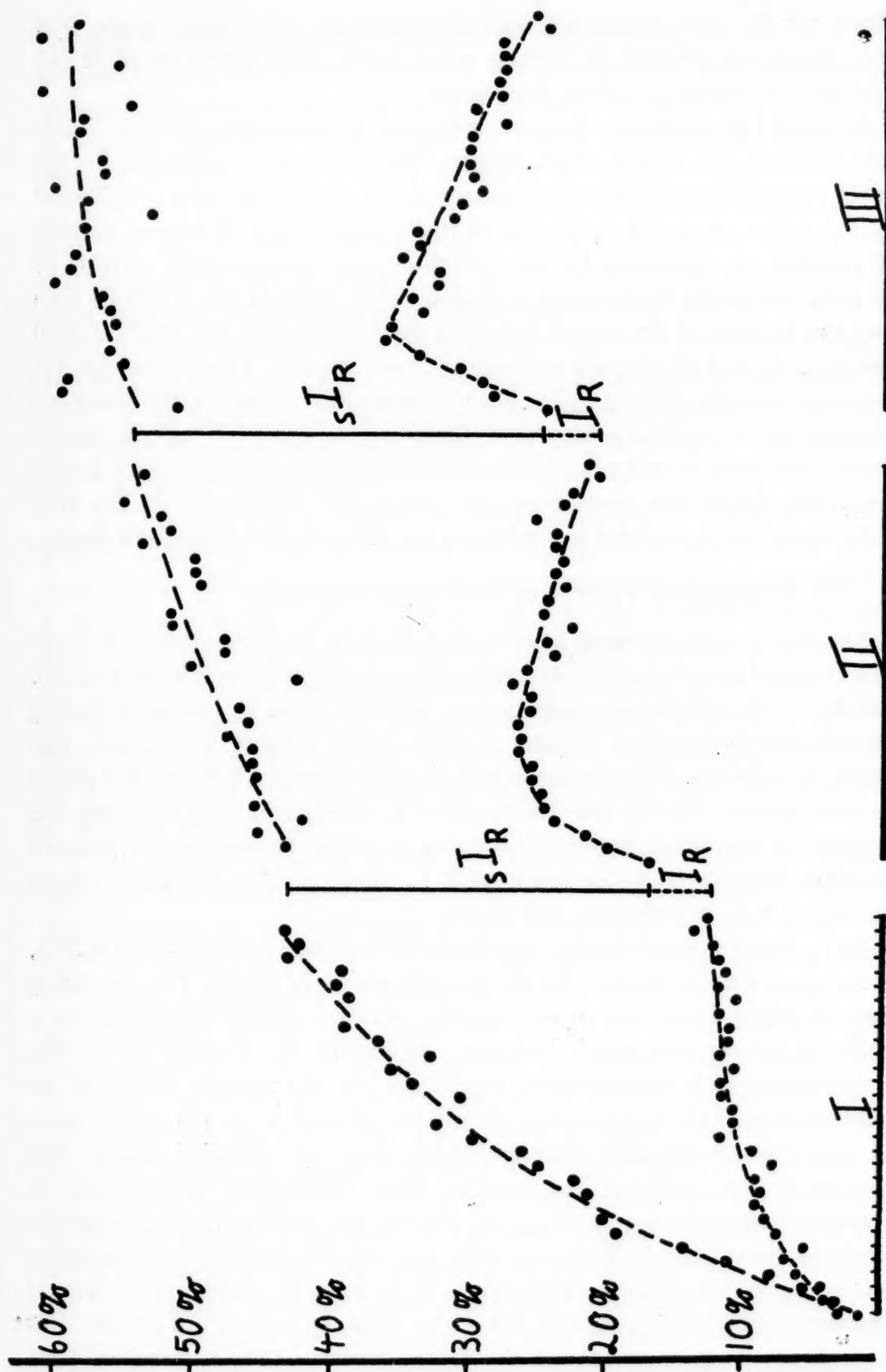


Fig. 2. Pursuit rotor performance as a function of massed practice (lower set of curves) and distributed practice (upper set of curves).

is that distributed practice is distributed sufficiently for I_R never to reach a concentration sufficiently high to enforce rest periods; before that happens, practice ceases and a rest is introduced by E which will allow all of the accumulated I_R to dissipate).

We are now in a position to put forward our own hypothesis regarding the sudden post-rest increment in performance. It is suggested that this rise in performance is due to the extinction of sI_R consequent upon the failure of sI_R to receive its appropriate reinforcement. This reinforcement, it will be remembered, consisted in the I.R.P.'s enforced by the high level reached by I_R . During the 10-minute rest, however, all of I_R has been dissipated, and consequently during the first minute or two after the rest pause I_R is accumulating again until it reaches its critical level. It is only when this point is reached, i.e., after a minute or two, that sI_R is reinforced. Until then, sI_R , in accordance with learning theory, should extinguish in view of the fact that *no reinforcement is forthcoming*.

This theory of post-rest increment is similar in some ways to an hypothesis put forward by Denny, Frisby, and Weaver (25) to account for the fact that groups of subjects switching from massed to distributed practice finally achieve as high performances as groups of subjects starting with distributed practice and going on with distributed practice. Their explanation is as follows: "Theoretically, if one considers, as we do, that the unconditioned stimulus for the establishment of conditioned inhibition is the massing condition, then when massing (US) is omitted by introducing distributed practice the conditioned inhibition, like other conditioned responses, should undergo extinction." Our own phrasing would be slightly different. It would be to the effect that if one considers, as we do, that the unconditioned stimulus for the establishment of conditioned inhibition is the occurrence of rest pauses enforced by the accumulation of reactive inhibition due to massing, then the omission of these enforced rest pauses due to the dissipation of reactive inhibition during rest, causes conditioned inhibition, like other conditioned responses, to undergo extinction.

It is important to be quite clear about the sense in which our theory is an alternative to the Bell-Ammons warm-up hypothesis. It is not suggested that there is *no* warm-up after rest; what we are suggesting rather is that warm-up effects do not account for *all* the post-rest increment in performance which is observed, and that, in fact, the major part of this increment is due to the extinction of sI_R . A complete theory of post-rest increment in performance thus requires three different concepts in addition to ordinary improvement through practice on the last pre-rest trial:

- (1) *Reminiscence* or the increase from the last pre-rest trial to the first post-rest trial.
- (2) *Warm-up* or the rapid rise in performance during the first few seconds of practice after rest. (Short-term increment, extending over 10-20" only).
- (3) *Extinction increment*, caused by the extinction of sI_R producing a relatively long and rapid rise in post-rest performance (long-term increment; extending over 60-90").

It is important to keep these three phenomena distinct as their theoretical derivation and their experimental determination are quite different.

4. THE EXISTENCE OF sI_R

Our theory for the explanation of post-rest increment in terms of the extinction of sI_R would clearly have very little value if any doubt existed about the development of sI_R as such. Ammons and Willig (26) report failure to find evidence for the existence of sI_R , and quote several other writers in support. (27, 28, 29, 30, 31, 32, 33).

The experimental arrangements under which sI_R failed to be uncovered were not, however, entirely free from criticism. Where pursuit rotor learning was used, distributed practice often included uninterrupted periods of work of as long as one minute. There is ample room during a minute for both I_R and sI_R to arise (26). Massed practice periods have not always been strictly massed; in the work of Adams and Reynolds (34) for instance, 5 second rest pauses were incorporated in their massed practice periods. Other writers again failed to take into account the extinction hypothesis put forward by Denny, Frisby and Weaver (35). In other studies the Alphabet Printing Task was used. This is not as suitable, in our experience, as pursuit rotor learning in studies of this kind. The various parts of the task are much more practiced before the first experimental trial, than are the components of the pursuit rotor task; it is difficult to make trials as continuously massed; lastly, the different difficulty level of the task at different parts of the alphabet creates considerable disturbance. Figure 3 shows the mean scores of 50 male Ss on the Alphabet Printing Task, carried out in the manner described by Schucker et al. (36) There were 3 sessions divided by 10 minute rest pauses; each session consisted of 10 consecutive 30-second trials. There is no warm-up effect, but rather a drop in performance following the first trial in each session; there is no evidence of I_R or reminiscence, the rise in performance after the rest pauses not being statistically significant. In the absence of I_R , we would not expect any sI_R to arise, and failure to find evidence for the latter on this task cannot, therefore, be taken too seriously (37). Altogether, the considerable differences between Ss in pre-experimental familiarity

with the tasks make it difficult if not impossible, to generalize or interpret findings. As a pencil-and-paper measure of inhibition, the Tsai-Partington Numbers Test, as adapted by Ammons, appears much less open to objection (38, 39).

LETTERS:

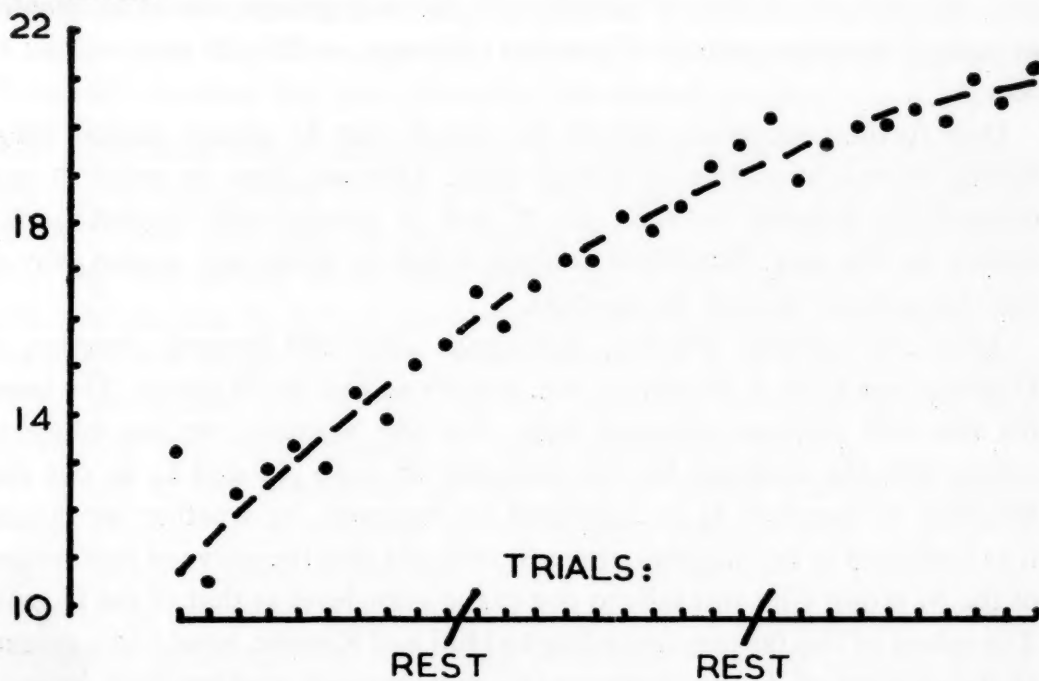


Fig. 3.

Improvement in the Alphabet Printing Task as a function of practice, showing failure of rest periods to produce reminiscence effects.

However valid these criticisms, it seemed more worth while to produce direct experimental evidence regarding the existence of g_{IR} . Figure 2 shows the outcome of an experiment specially conducted for this purpose. The set of curves in the lower half of the figure has already been discussed. The set of curves in the upper half of the figure consists of 10 second trials separated by 30-second rest periods; records of 25 university students were averaged in order to obtain the results reported. Each 10-second period was preceded by $2\frac{1}{2}$ seconds practice during which no score was kept. This was done in order to make comparable the 10-second period of work in the D group (distributed practice) with corresponding 10-second periods of work in the M group (massed practice). In the M group each 10-second period would begin with the subject already in the middle of his task. If, in the D group, the subject were instructed to begin work at the beginning of the 10-second period, at least a second or more would be lost in his getting the stylus on to the turn-table, beginning to move it, etc. The $2\frac{1}{2}$ -

second periods preceding each trial, while not scored, nevertheless furnish an opportunity of practice for each subject, and were therefore included in arriving at an estimate of the total amount of time spent in practice by the D subjects. For this reason, therefore, there are only 24 trials for the D group to compare with every 30 trials in the M group. This ensures that the amount of time of practice for the two groups would be identical in each of the three periods of practice (300 secs. = 30×10 secs. = $24 \times 12\frac{1}{2}$ secs.).

One further exception should be noted—the D group started off by having three consecutive 10-second trials. This was done in order to make comparison possible between the N and D groups with respect to their ability on the task. Statistical analysis failed to show any reason why the null hypothesis should be rejected.

After 300 seconds practice, and again after 600 seconds practice, the D group was given a 10 minute rest, exactly as had the N group. The reason for this will become apparent later. For the moment, we are concerned rather with the evidence for the existence of both sI_R and I_R in our data. Whether we measure I_R as suggested by Ammons, or whether we measure it as indicated in the diagram, there is no doubt that the curve of performance of the M group after rest fails to rise to the same level as that of the D group. The extent of this failure, according to Hull and Kimble, would be a measure of the amount of sI_R , or permanent work decrement, and has been indicated as such in Figure 2.

To assess the significance of the difference in performance between the M and D Groups after the two rest intervals appeared to be a task of supererogation as there was practically no overlap between the two sets of scores. However, analysis of variance was performed both on the original scores and on the square root transformation of the scores. (This transformation appeared necessary as there is a linear relationship between average score and variation about the average. The square roots as tested with Bartlett's test were found to vary homogeneously). Differences were significant well beyond the .001 level, thus leaving little doubt about the reality of sI_R .

I_R will be seen to produce much smaller effects than sI_R . These effects however, are also fully significant statistically as has been shown in a previous publication. We may conclude therefore that our data support the Hullian theory regarding the existence of both I_R and sI_R .

5. EXPERIMENTAL FINDINGS

The theory outlined above makes it possible for us to make certain

predictions which can be experimentally tested. These hypotheses will be stated seriatim, together with a brief discussion showing how they derive from the general theory, and a demonstration of relevant experimental findings.

H.1: A rest of sufficient length to allow the total amount of reactive inhibition accumulated during preceding practice to dissipate will result in a long-term post-rest increment in performance following massed practice, but not following distributed practice.

This hypothesis follows directly from our general theory. Massed practice allows I_R to accumulate to the point where involuntary rest pauses are enforced; these then lead to the growth of sI_R . Lack of reinforcement during the first minute or so after rest leads to the extinction of sI_R . In distributed practice I_R is not allowed to reach a sufficiently high level for involuntary rest pauses to appear, and therefore no sI_R is generated. Consequently, we should not be able to find anything corresponding to the extinction of this (non-existent) sI_R after distributed practice.

For proof of this hypothesis we may turn to Figure 2 which contrasts the performance of 50 subjects during massed practice with that of 25 subjects during distributed practice. Details of the experiment have already been given in an earlier section of this paper. Simple inspection of this diagram will show that our hypothesis is verified. There is nothing in the curves of distributed practice following the two 10-minute rest intervals R_1 and R_2 which remotely resembles the steady, long-continued rise which is so noticeable under conditions of massed practice. There is, indeed, in both cases a rise from the first to the second 10-second practice period in the distributed practice curve, but this rise is not continued, as it is in the case of the massed practice curves, for a period of 60-90 seconds. It seems likely, therefore, that what we are dealing with in the curve of distributed practice is warm-up effect, producing a rise in performance continued for 20-seconds at most. Such a rise is also apparent in a figure given by Adams (40) in his paper on "Warm-up decrement in performance on the pursuit rotor"; as the rest pauses in his case were of 24 hours duration rather than of 10 minutes duration, the warm-up effect is rather stronger. Nevertheless, in his case also it appears to reach its maximum after 20 seconds or so.

H.2: Post-rest increment due to the extinction of conditioned inhibition through a failure of reinforcement will be more rapid and more extensive after the second rest period (R_2) than after the first (R_1).

This prediction follows immediately from the fact that extinction phenomena are more easily obtained on the second or subsequent occasion than

they are on the first occasion; this is a well-known principle of conditioning (41). If the rapid rise in our massed practice curves after rest is indeed due to extinction, then repetition of this extinction should make it both more rapid and marked.

Casual inspection of Figure 2 indicates that this hypothesis is also verified. 110 seconds are required to reach the top of the curve after R_1 , but only 60 seconds after R_2 . Thus, the rate of increase has almost doubled from one curve to the other. The amount of increase is 10 % after R_1 and 12 % after R_2 . However, casual inspection in this case is clearly not enough to establish the significance of the observed phenomena and a proper test becomes requisite.

Such a test requires the use of analysis of variance and necessitates the splitting up of the differences between the three series into differences in (1) *Level* (average performance in the entire series); (2) *Gradient* (rectilinear regression of score on run); (3) *Curvature* (progressive change in the regression rate over the series); and (4) *Chance Fluctuations* (42). As means and variances were related in a simple rectilinear fashion, the original units of measurement had to be transformed. Square roots were accordingly taken and tested for homogeneity with Bartlett's test. This gave a chi square value of 95.827 for 89 d.f. and a P of .29. (The alternative of a logarithmic transformation was also considered but found unsuitable).

The total variance of the transformed scores breaks down into three main parts: the variance between persons, which, although substantial, is of no particular interest to this enquiry; the variance between runs, which is to be analysed in detail; and the remainder due to inconsistencies in individual performance on different runs, which provides an estimate of mean square variance due to chance. Results are given in Table I. Average performance on all the runs is 2.52 in Series 1, 4.65 in Series 2, and 5.34 in Series 3. The massive mean square variance "between levels" given in Table I shows that the differences between these averages are highly significant. The regression of score on run is +.0565 in Series 1, +.0039 in Series 2 and -.0161 in Series 3. The analysis shows that these gradients differ significantly. Curvature is significant and similar in all three series, but there is no substantial significance attached to the apparently greater steepness in post-rest increment in Series 3 as compared with Series 2. This failure of the transformed scores to reach significance may in part at least be a function of the transformation of scores undertaken to fit the requirements of the statistical analysis. The very close fit of the curves to the original observations as shown in Figure 2 suggests (but does not prove) that on repetition the same effect would be obtained again.

TABLE I
Analysis of the variance of the transformed scores

Source of Variance	Sum of squares	d.f.	m.s.v.
Main components:			
Variance between persons	5,831.75	49	
Variance between runs	7,142.31	89	
Residual error variance	5,783.33	4,361	1.326
<i>Total</i>	18,757.39	4,499	
Detailed analysis of the variance between runs:			
Variance accounted for by			
(a) differences between levels of performance in different series	6,482.92	2	3241.460
(b) mean regression of score on run, all series combined	73.60	1	73.599
(c) differences in rates of regression in different series.....	316.37	2	158.185
(d) general pattern of departure from a uniform regression rate, observed in all three series	239.07	28	8.538
(e) variations in pattern between series	30.35	56	.542
<i>Total</i>	7,142.31	89	

H.3: A rest, following massed practice continued for a sufficiently long period to allow reactive inhibition to reach its critical level, will result in extinction increment in performance; a rest following practice not sufficiently long continued to allow reactive inhibition to reach its critical level will not result in extinction increment in performance.

It will be clear from the general theory we are investigating that no conditioned inhibition will be generated until the hypothetically enforced involuntary rest pauses which act as reinforcement begin to occur. This, according to the theory, will not be until reactive inhibition reaches its critical level. From the available work of Kimble (43, 44) Ammons (45) and others it would appear that in pursuit rotor learning, the beginning of conditioned inhibition would be around the 90 seconds period, although it is impossible at the moment to say with any degree of exactness where precisely this point should be located. (There is evidence to show that the point is not a constant, but depends on degree of motivation, and it is also likely that there will be individual differences in this respect).

In spite of these doubts about the exact moment of reaching the critical level, our theory would lead us to expect that after two minutes of massed practice little or no extinction increment would be observed, while after long periods, such an increment should make its appearance. Figure 4 shows the results of an experiment which is relevant to this point. 120-second periods of massed practice were separated by 5-minute rests; the data were obtained from 20 women between 18 and 30 years of age (average 25 years). Casual inspection will indicate that there is no evidence for the extinction increment in performance after the first rest pause, but that a very strong increment of this kind is observed after R_2 , R_3 , R_4 , etc. As predicted in H_2 , this phenomenon becomes more clearcut and more extensive after later rest pauses; thus, the increment is steeper and more marked after R_3 than after R_2 . (After R_6 , however, this tendency begins to disappear; a theoretical explanation to cover this phenomenon will be given later).

The reader may find an apparent contradiction in this discussion. If I_R does not reach its critical level during the first practice period, and if I_R completely dissipates during the first rest period, then it would seem that in the second practice period the build-up of I_R will start again *de novo* so that at the end of the second practice period there should again be a failure of I_R to reach its critical level. It is conceivable that such a state of equilibrium might occasionally occur, but what is more likely to have happened is this. Towards the end of the first practice period, I_R reaches its critical level and a limited, rather small amount of sI_R is built up. (As evidence it may be noted that the last thirty seconds practice during the first period fall distinctly below the previous level reached. This would make it appear that the asymptote of I_R has been reached after approximately 90 seconds). A further increment of sI_R is generated by the rest pause following the first practice period. Thus, we do not start *de novo* on the second and successive trial sessions. The possibility must also be considered that the slight amount of sI_R generated during the first period will summate with I_R to form \dot{I}_R . This \dot{I}_R would reach an asymptote rather earlier than would I_R without the addition of sI_R . This summation of sI_R and I_R to form \dot{I}_R would produce a critical level in the accumulation of inhibition earlier and earlier.

Fortunately the part of this argument relating to the summation of I_R and sI_R is not critical for our hypothesis. There has been a considerable amount of criticism by Koch (46), Osgood (47), Gleitman et al (48), of this hypothesis, and indeed, in terms of Hull's own system, it does not seem reasonable to summate a drive (I_R) and a habit (sI_R). Osgood (49) and

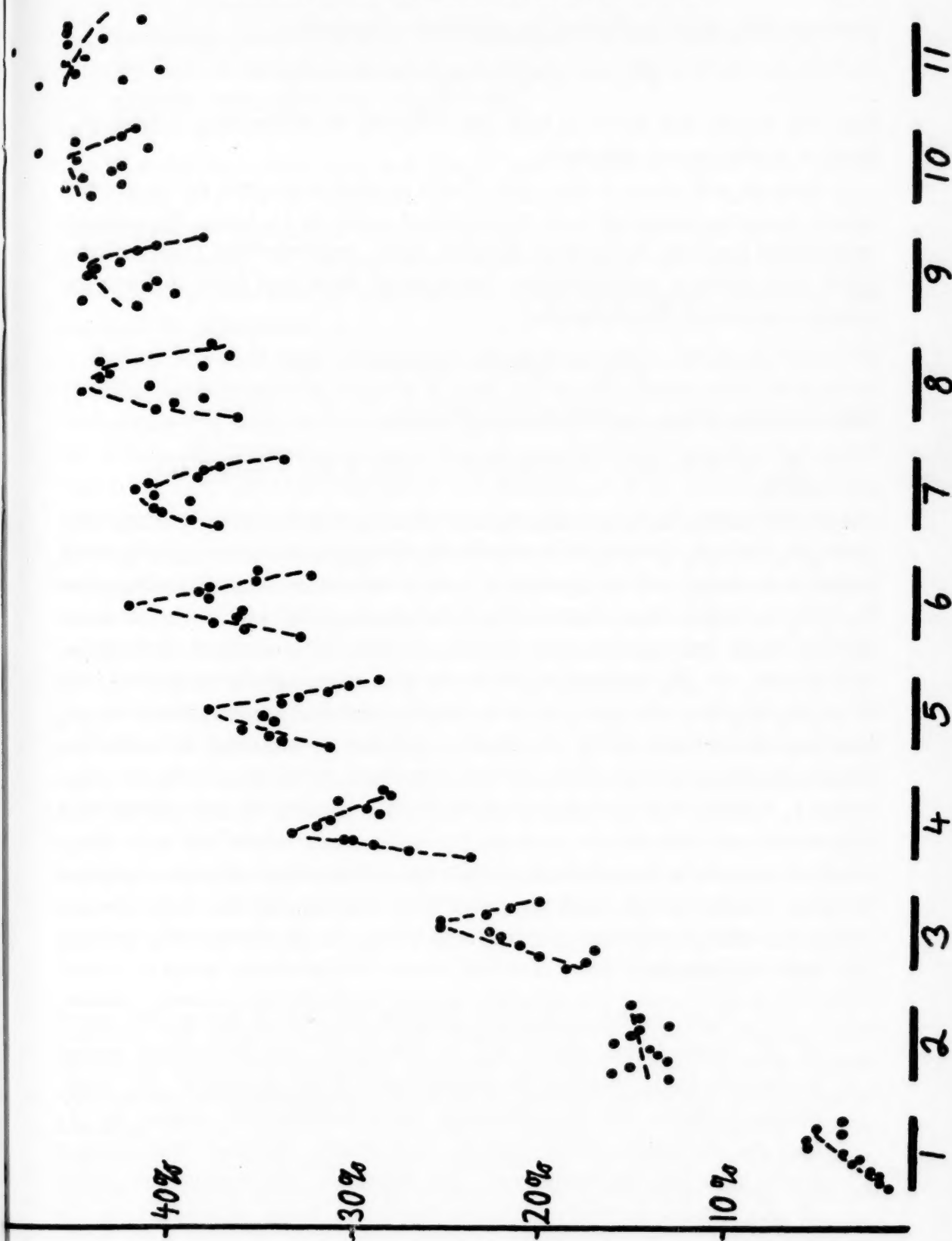


Fig. 4. Reminiscence and extinction increment as a function of rest pauses introduced after two minute massed practice trials on the pursuit rotor.

Zeaman (50), have suggested an alternative formula:

$${}_s\bar{E}_R = ({}_sH_R - {}_sI_R) \times D - I_R$$

but this would still leave us with the difficulty of subtracting a drive (I_R) from a performance potential.

A formula still more in line with Hull's explicit statement of his general theory has been suggested in an unpublished paper by G. Jones. He suggests subtracting negative habit from positive habit, negative drive from positive drive, and using a multiplicative function of habit and drive to form the reaction potential. Symbolically:

$${}_s\bar{E}_R = ({}_sH_R - {}_sI_R) (D - I_R)$$

This formula when multiplied out gives us:

$${}_s\bar{E}_R = (D \times {}_sH_R) - (D \times {}_sI_R) - ({}_sH_R \times I_R) + ({}_sI_R \times I_R)$$

From this certain consequences would seem to follow. If we assume that both ${}_sH_R$ and ${}_sI_R$ grow in accordance with a negatively accelerated growth function reaching a final asymptote, and if we assume that the asymptote for ${}_sH_R$ is higher than that for ${}_sI_R$ (two assumptions which receive much support from the experimental literature) then we can argue as follows. The growth of ${}_s\bar{E}_R$ is determined in its first stage almost exclusively by $D \times {}_sH_R$ (I_R has not yet grown to any extent and consequently no ${}_sI_R$ has been developed). As I_R develops we get the second stage in which the reaction potential is determined by the expression $(D \times {}_sH_R) - (I_R \times {}_sH_R)$. When I_R reaches a critical point, i.e., when it is equal to D , involuntary rest pauses are enforced which produce ${}_sI_R$ and we now reach the third stage in which reaction potential is determined by all four elements in the expanded formula. Finally as ${}_sH_R$ and ${}_sI_R$ reach their asymptotes, the only element which can change reaction potential will be I_R . From this we may develop our next hypothesis.

H.4: Near the beginning and towards the end of the growth curve of ${}_s\bar{E}_R$ reminiscence effects will be influenced and determined almost exclusively by I_R . During the middle part of the growth of ${}_s\bar{E}_R$ reminiscence effects will be influenced to a considerable degree by ${}_sI_R$ and its extinction. Consequently correlations between reminiscence scores obtained relatively early and relatively late during the growth of ${}_s\bar{E}_R$ should correlate together positively, being dependent on the dissipation of I_R . Similarly reminiscence scores derived from trials occupying the middle part of the growth curve of ${}_s\bar{E}_R$ should correlate

positively, being strongly determined by the extinction of ${}_sI_R$. The two sets of reminiscence scores should show a much lower correlation, possibly even zero or a negative one.

The point here made is a very simple one. It may be stated most briefly by saying that the development of conditioned inhibition interferes with the proper determination of reminiscence score in all trials except those at the beginning, i.e., before conditioned inhibition has been developed, and those at the end of practice, i.e., when conditioned inhibition has reached its asymptote.

It may be asked how this can be when our theory effectively splits all inhibition into two parts, namely, I_R and ${}_sI_R$, or temporary work decrement and permanent work decrement. Our measure of temporary work decrement is, in fact, the difference between the last pre-rest trial and the first post-rest trial; how can this measure of the dissipation of I_R be affected by the extinction of ${}_sI_R$ taking place *after* our measurement of reminiscence has been made? The answer to this question, of course, is that the *point* at which measurement takes place is not a geometrical point, i.e., one having no extent, but is, in fact, an average performance over a time interval of 10 seconds. In terms of our theory, and in terms of Figures 2 and 4 as well, this time interval appears to be an exceedingly dynamic one in which a considerable amount of extinction is taking place. Our measurement of reminiscence will only be uninfluenced by ${}_sI_R$ if it could be taken over a very small period of time, such as a fraction of a second. As this is clearly impossible, the consequences delineated in H.4 must follow (51).

Do the predicted results actually occur? In an attempt to answer this question, reminiscence scores were obtained after each of the ten imposed rest intervals shown in Figure 4. These were then correlated and a factor analysis performed (52). In terms of our theory we should find high correlations among reminiscence scores following the first and the last rest pauses; positive correlations among reminiscence scores following intermediate rest pauses; and low or zero correlations between the two sets of reminiscence scores. Table II gives the results of a factor analysis carried out on the intercorrelations. In view of the high standard errors, factor loadings of less than $+ .30$ have been omitted. Two orthogonal factors appear after rotation, carried out according to the dictates of Thurstone's simple structure criterion; these two factors correspond rather well to those demanded by our theory. While the numbers involved in this experiment are small, the results appear in considerable agreement with our hypothesis. (The average correlation among the group $R_1, R_2, R_3, R_9, R_{10}$,

is .36; that between R_4 , R_5 , R_6 , R_7 , R_8 is .27. The correlation between the two groups is -.10).

TABLE II

	I	II
R_1	.57	—
R_2	.76	—
R_3	.49	—
R_4	—	.65
R_5	—	.37
R_6	—	.68
R_7	—	.40
R_8	—	.47
R_9	.66	—
$R_{1'}$.60	—

6. DISCUSSION

As was mentioned at the beginning of this paper, the experiments here described were carried out in an attempt to decide on the most suitable method for measuring reactive inhibition through the reminiscence effect. The problem which most directly affects this measurement appears to be the so-called "warm-up" effect, which is considered to interfere to a considerable extent with the measurement of reminiscence, and which is being compensated for by many investigators in a somewhat wholesale manner. Our investigation has shown that while the "warm-up" effect does exist, it is of considerably less magnitude than previous theories had led one to assume. Thus, the Ammons correction for warm-up effect is far too drastic and is likely to do more harm than good. If there is to be any correction for warm-up effect, then it appears to be essential that further research should be done, particularly under conditions of distributed practice, where the warm-up effect is not likely to be confounded with the extinction increment effect described in this paper. In particular, the following three questions arise:

- (1) Are there individual differences in warm-up, and if so, how are they related to personality factors?
- (2) What is the precise rate of growth of warm-up decrement, and how is it related to the stage of practice reached?
- (3) To what extent is warm-up itself a learned phenomenon, e.i., to what extent do repeated pauses teach the subject to warm up more quickly and more expertly?

Until answers are obtained to these questions it would not be advisable to correct for warm-up effects. A more advisable procedure might be to

minimize warm-up effects through the procedures suggested by Irion and others.

While thus warm-up effects appear to be less important than had been supposed, another effect which had not hitherto figured in the experimental literature appeared to interfere to a considerable extent with the measurement of reminiscence. This factor, called the "extinction increment" because of its hypothetical cause, namely, the extinction of sI_R through non-reinforcement, led to an increment in performance which appeared to vitiate the measurement of reminiscence, particularly in the middle stages of practice; in the early and late stages of practice this effect appeared to be of negligible importance. The obvious deduction from this finding would seem to be that there are certain favourable points in the learning curve at which rest pauses may, with advantage, be included if the measurement of reminiscence is the aim of the experiment.

From the broader theoretical point of view, the results of the experiments described here all appear to be deducible from Hullian learning theory, and to the extent that this is so it must undoubtedly be admitted that they strengthen this particular theoretical system. Indeed the writer was surprised to find how closely experimental facts could be integrated with theoretical deductions. Nevertheless, there are certain weaknesses which should be remedied before the hypotheses outlined in this paper can be readily accepted. The main weakness of the general theory appears to be its lack of quantification. We have a rough notion of the type of curve followed by conditioned inhibition and by reactive inhibition, but it is impossible from the literature to derive a formula for these two curves which alone would make possible an exact quantitative prediction of our results. This lack of quantification of intervening variables and hypothetical constructs runs through the whole of psychology, of course, and is not found in Hullian learning theory alone; nevertheless, until the qualitative kind of deduction tested here is replaced by a more quantitative kind of deduction, so long will it be impossible to exclude alternative theories with a very high degree of certainty. Quantitative studies of this type are being carried out at the moment, and it is hoped that a more adequate numerical formulation of the hypotheses here given may be possible later on. Until then, we must remain content with noting the power of learning theory to generate verifiable deductions on a qualitative level.

SUMMARY

The traditional treatment of "warm-up" effects occurring after rest periods is in terms of the loss of muscular and ideational set, a loss which has to be made good during the first few seconds of renewed practice. There are experimental grounds for doubting

whether such "warm-up" effects, although undoubtedly existing, are capable of explaining all the observed post-rest performance increments, and an additional theoretical construct is deduced from learning theory to account for these phenomena. This construct (extinction increment) refers to the hypothetical extinction of conditioned inhibition during practice, after a rest pause during which reactive inhibition (which serves as a reinforcement for conditioned inhibition) has been dissipated. Several deductions are made from this general theory, and results are given from experiments verifying these deductions. The results of this set of experiments make possible an improvement in the measurement of reactive inhibition through the reminiscence effect by clarifying the conditions under which correction for warm-up should be applied.

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51. We may find here an explanation also of the fact that the extinction increment appears to vanish during the last few trials. In terms of conditioning theory, repeated extinctions become very much more rapid until at the end they would be expected to be practically instantaneous. Thus it might be argued that the extinction increment occurs wholly during the first 10 seconds trial of the last session. An alternative hypothesis to account for this effect may be related to asymptote or "ceiling" effects. As the asymptote of performance is reached there is less and less room for extinction increment to occur. Both these hypotheses may, in fact, be correct and supplement each other. One possible deduction to be made from the first hypothesis would be that reminiscence scores include more and more extinction increment and become larger in later sessions. An opposite prediction would be made in terms of our second hypothesis. Unfortunately it is impossible to make an empirical test of this, as we would require a more rational unit, such as Hull's *habs* instead of the raw scores we have been using. Such refinements must await the construction of a more quantitative theory that we have available at present.
52. The writer is indebted to Mr. K. H. Star for carrying out this analysis.

THE ADDITION OF SUBJECTIVE PROBABILITIES:

The Summation of Estimates of Success and Failure

BY

JOHN COHEN, E. J. DEARNALEY AND C. E. M. HANSEL

University of Manchester

INTRODUCTION

If a man estimates that he would *succeed* in three out of five attempts at a given task, can we infer that if we had asked him instead how many times he would *fail*, he would have answered twice out of five? More generally if we ask him to predict his successes *or* his failures in N attempts, would he predict n_1 successes *or* n_2 failures, where $n_1 + n_2 = N$, according to the question we ask him? This is the specific problem to be treated in this paper but before discussing it let us consider the context in which it arises.

We have previously (1) demonstrated certain systematic tendencies in measures of the subjective probability (ψ)¹ of the occurrence of an event. The measure extends from 1, through 0.5 to 0 as one's state of mind ranges from complete certainty that the event will occur at one extreme, through maximum doubt whether it will occur or not, to complete certainty that it will not occur, at the other extreme. In precisely the same way, we can measure our subjective probability in the correctness of our beliefs and judgments. Such measures have been obtained in relation to estimates of the proportion of tasks correctly performed in the past (or to be performed in the future); estimates of the outcome of future events; attempts at skilled or dangerous tasks etc. In one class of measures there exists a corresponding *a priori* or mathematical probability. In certain circumstances there is a tendency for implicit subjective probability, as the subjects increase in age, to approximate more closely to mathematical expectation as, for instance, in estimating the relative frequencies in samples drawn from a given population. In other circumstances, the subjective probabilities seem to obey rules of their own which differ from those of mathematical probability. An example of this is the tendency, when presented with a series of binary events, to predict the outcome which has occurred less frequently in the past. We have called this the phenomenon of pseudo-subjective dependence (1). Here we are primarily concerned

¹ As in previous publications the symbol ψ denotes subjective probability.

with the question whether measures of subjective probability have additive properties corresponding to those of mathematical probability.

In an earlier study we investigated the relationship between estimates of success in performing a task and the range within which the estimates were made (2). For example, one might ask a person 'how many times would you succeed in a given task if you had 5 (or 10, or 100 etc.) attempts?' This problem had itself arisen in a study of risk-taking by bus drivers at different stages of their training (3). We found that estimates made within different ranges are not transposable when made by children aged 9-10; an effect which is less marked at the age 13-14. In other words, we cannot project an estimate say of '4 out of 5' made by a ten-year-old child as equivalent to a direct estimate of '80 out of 100'. We inferred that estimates of success by adults would probably be transposable.

These results raise certain further problems, namely:

- (a) Does the non-transposability which characterises estimates of success also characterise estimates of failure made by children of the same age?
- (b) Are estimates of success and failure, when made independently, additive in the sense that mathematical probabilities of all possible outcomes of an event add to unity? In other words, is the additive theorem a feature of subjective as of mathematical probability? Does the identity $\psi a + \psi \bar{a} = 1$ hold good for subjective probability?

PROCEDURE

The procedure in this experiment was identical with that followed in the previous study (2) except that now the children estimated failure instead of success. They were asked to imagine that they were throwing a small ball at a target through a circular aperture 4ft. away. Both aperture and target were set on an easel in front of the children and one child illustrated the task by making a trial throw of the ball at the target. Apertures of eight different magnitudes were presented:

Feet	2.25	1.75	1.50	1.25	1.00	0.75	0.50	0.25
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Each child received a booklet of 6 pages and on each page he recorded his estimates of the number of times he would fail to hit the target through the various apertures. Each estimate related to a given number of possible attempts viz. 1, 5, 10, 100, and 1000. Thus the questions which each child had to answer were of the form: "You have a thousand shots at the target. How many times do you think you will miss it?" The children were divided into five sub-groups, each sub-group receiving the ranges (e.g., 'out of 5', 'out of 10', etc.) in a different order. The eight apertures were also presented in five random orders (A...E). The order in which the ranges and apertures were presented is shown in Table I.

TABLE I
Order of Presentation of Ranges

Order of apertures	<i>Sub-groups of children</i>				
	I	II	III	IV	V
A	0-1	0-5	0-10	0-100	0-1000
B	0-1000	0-100	0-5	0-10	0-1
C	0-100	0-10	0-1	0-1000	0-5
D	0-5	0-1000	0-100	0-1	0-10
E	0-10	0-1	0-1000	0-5	0-100

Two of the apertures (1.50ft. and 0.75ft.) were presented twice to provide a measure of the consistency of the estimates. Each child thus made 50 estimates, 10 estimates within each range.

On the sixth page, the following questions were presented and explained verbally:

- (i) Which of the five kinds of question (i.e., which kind of range) did you find it easiest to answer?
- (ii) Which did you find hardest to answer?
- (iii) Which of the five questions (ranges) would you use if you were explaining to a friend how hard it is to throw the ball at the target through the aperture (1ft. diameter) in front of you?

The subjects consisted of two groups of school-children, 83 aged 9-10 years, and 58 aged 13-14 years.

RESULTS

The repeated estimates given for the target areas 1.75 and 0.45 sq. ft. are remarkably consistent especially when considered in terms of the ranges within which they are made. The estimates for the repeated targets

are much more similar than the estimates for either of these two targets and adjacent targets. These results closely resemble those obtained for the estimates of success.

Transposability of ψ_F

The median estimates of failure, expressed as ψ values, are shown in Tables II and III for the younger and older age groups respectively.

Medians rather than means were taken because they offset the extreme values given by a very small proportion of children.

TABLE II
Median subjective probabilities of failure (ψ_F)
 $N = 83$ (9-10 years)

Range	Areas of target (sq.ft.)							
	3.95	2.40	1.75	1.20	0.80	0.45	0.20	0.05
<i>Out of:</i>								
one	0.03	0.02	0.03	0.06	0.04	0.40	0.66	0.95
five	0.02	0.03	0.05	0.10	0.24	0.39	0.56	0.84
ten	0.01	0.03	0.05	0.15	0.21	0.31	0.49	0.76
a hundred	0.00	0.01	0.03	0.05	0.10	0.15	0.33	0.80
a thousand	0.00	0.00	0.01	0.02	0.02	0.04	0.08	0.20

TABLE III
Median subjective probabilities of failure (ψ_F)
 $N = 58$ (13-14 years)

Range	Areas of target (sq.ft.)							
	3.95	2.40	1.75	1.20	0.80	0.45	0.20	0.05
<i>Out of:</i>								
one	0.05	0.08	0.06	0.07	0.12	0.25	0.64	1.00
five	0.02	0.02	0.04	0.09	0.16	0.31	0.55	0.72
ten	0.01	0.02	0.02	0.05	0.10	0.28	0.45	0.80
a hundred	0.00	0.00	0.01	0.04	0.06	0.08	0.20	0.67
a thousand	0.00	0.00	0.00	0.01	0.01	0.03	0.08	0.70

After carrying out the experiment on ψ_S we envisaged two possible outcomes of a study of ψ_F . The ψ_F values would either increase with size of range in contrast to the corresponding decrease in values of ψ_S or they would also decline. In fact we find that the estimates of failure, expressed as subjective probabilities, also diminish with increasing size of range. There do not seem to be any age differences in this effect as in estimates of success (see Figs. 1 and 2). It follows therefore that we have no evidence for the transposability of ψ_F values in different ranges between the ages of 9 to 14 years.

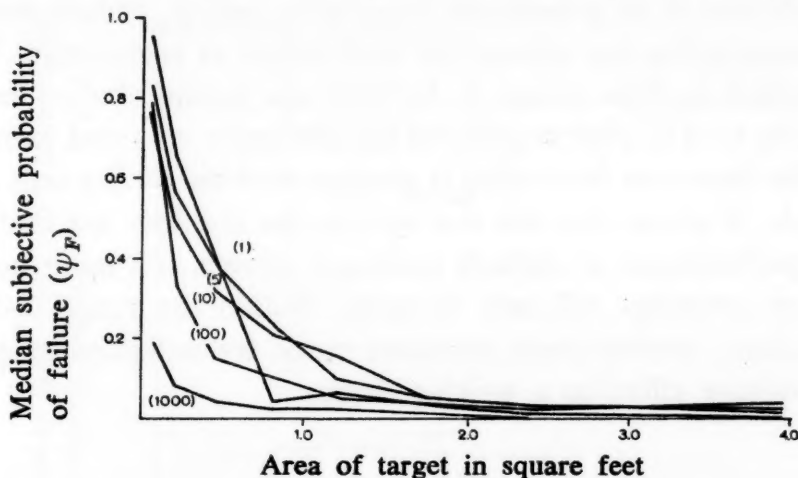


Fig. 1. Subjective probabilities of failure based on estimates at different ranges in relation to area of target ($N = 83$ children aged 9–10 years)

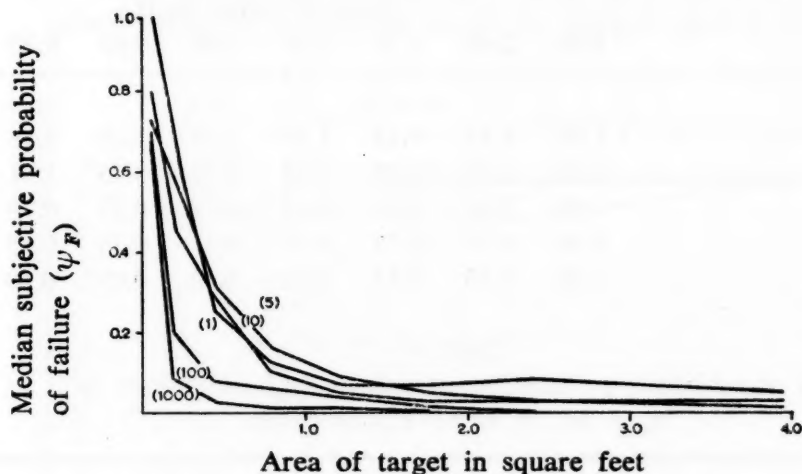


Fig. 2. Subjective probabilities of failure based on estimates at different ranges in relation to area of target ($N = 58$ children aged 13–14 years)

The additive character of ψ_S and ψ_F

We turn now to the question whether the estimates of success and failure are additive. It is obviously difficult to determine this additive character if we study a single group of subjects, for they could simply subtract their estimates of success (or failure) from the total number of estimates in order to estimate their failures (or successes). We have therefore to compare the ψ_S values of one group with the ψ_F values of another. In the present experiment the children estimating failure were an entirely different group from those who in the previous study, estimated success; they belonged to the same age groups and were of comparable social status and educational level (2).

In Tables IV and V we present the sums of ψ_s and ψ_F values obtained from the corresponding age groups for each target at each range. These values are plotted in Figs. 3 and 4. In both age groups the ψ_s and ψ_F values obviously tend to sum to unity on the extremely easy and extremely hard tasks. The departure from unity is greatest near the 50 per cent point. In other words, it seems that the less certain the children are that their estimates of performance at difficult tasks are correct, the more unlikely it is that these estimates will add to unity. Within the range 0-5, the ψ_s and ψ_F values, derived from estimates made by independent groups display the additive effect to a striking degree.

TABLE IV
Sums of subjective probabilities of success (ψ_s) and failure (ψ_F)
 $N_s = 63$, $N_f = 83$, (9-10 years)

Range	Area of target (sq.ft.)							
	3.95	2.40	1.75	1.20	0.80	0.45	0.20	0.05
<i>Out of:</i>								
one	1.04	1.11	1.12	1.14	1.13	1.19	1.16	1.11
five	1.00	0.98	0.98	0.96	0.99	0.95	1.01	1.00
ten	1.00	1.00	0.92	0.85	0.94	0.77	0.79	0.86
a hundred	0.99	0.91	0.73	0.55	0.41	0.35	0.45	0.83
a thousand	1.00	0.10	0.11	0.12	0.12	0.09	0.10	0.21

TABLE V
Sums of subjective probabilities of success (ψ_s) and failure (ψ_F)
 $N_s = 68$, $N_f = 58$, (13-14 years)

Range	Area of target (sq.ft.)							
	3.95	2.40	1.75	1.20	0.80	0.45	0.20	0.05
<i>Out of:</i>								
one	1.10	1.14	1.10	1.11	1.15	1.05	1.26	1.09
five	1.01	0.99	0.99	0.97	0.96	0.92	0.95	0.86
ten	1.00	0.99	0.97	0.92	0.88	0.82	0.84	0.92
a hundred	1.00	0.98	0.91	0.84	0.67	0.53	0.45	0.77
a thousand	1.00	0.91	0.87	0.62	0.61	0.33	0.28	0.75

What explanation can be offered for the departure from unity of the sums of the ψ_s and ψ_F values in ranges other than 0-5? If the decline in ψ_s with increase in range is due to the fact that subjective probabilities are intrinsically altered with variations in the range within which they are expressed, then values of ψ_F should increase whenever values of ψ_s decrease and *vice versa*. In fact the ψ_F values, like the ψ_s values, undergo

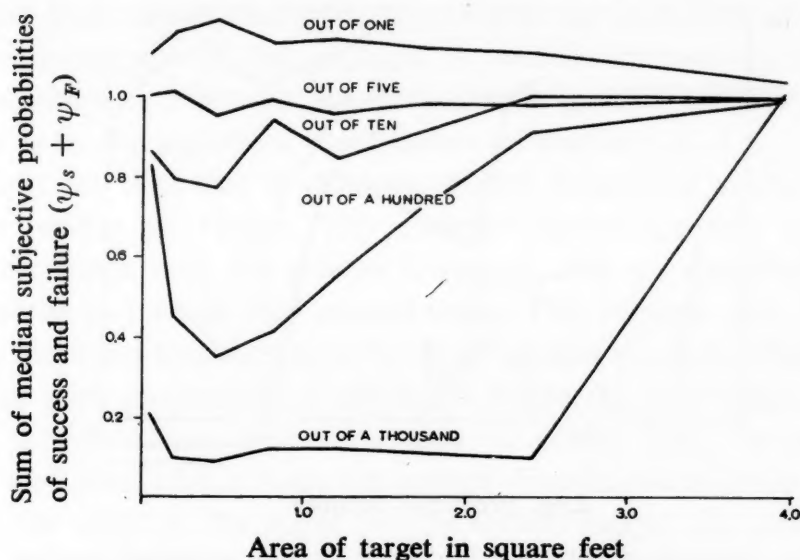


Fig. 3. Additive properties of subjective probabilities of success and failure estimated at different ranges by children aged 9-10 years

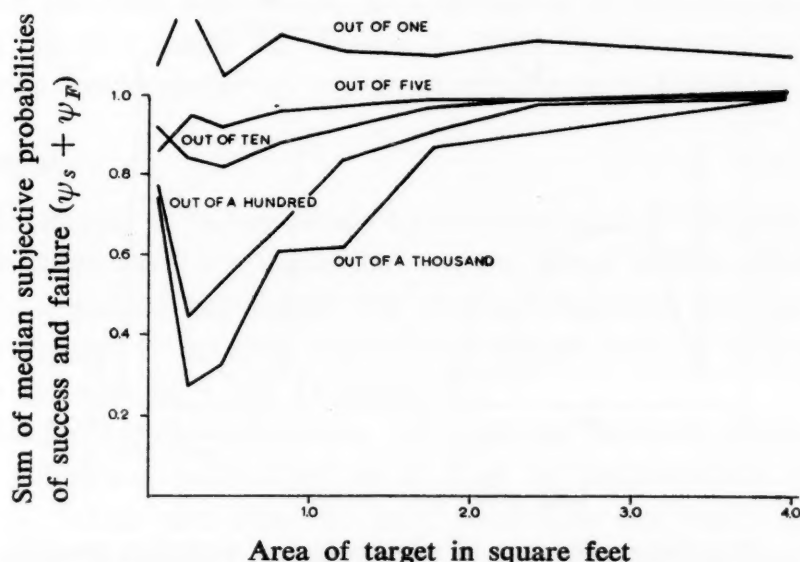


Fig. 4. Additive properties of subjective probabilities of success and failure estimated at different ranges by children aged 13-14 years

a decline with increasing range. So this cannot be due to a primary change in subjective probability but must be attributable to the influence of the size or difficulty of the number used in recording any particular ψ value in relation to the range. This interpretation is supported by the following argument. If the percentage reduction in ranges larger than 0-5 on the basis of estimates made in that range, were not at all due to the size of the number recorded, then the percentage reduction for any number

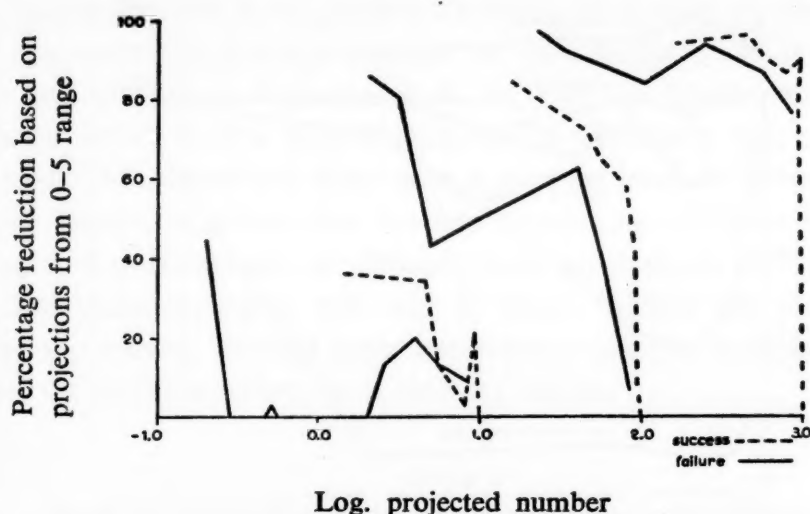


Fig. 5. Percentage reduction in relation to log. projected number.
Children aged 9-10 years

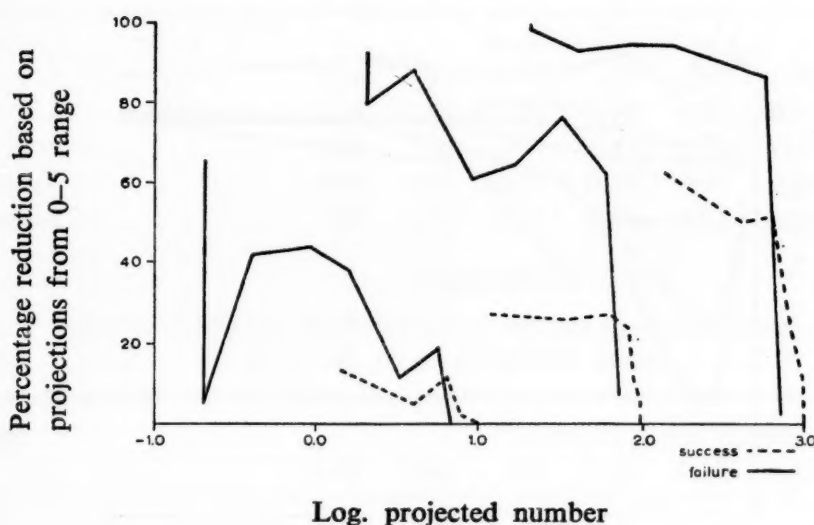


Fig. 6. Percentage reduction in relation to log. projected number.
Children aged 13-14 years

recorded should not vary from ranges 0-10 to 0-1000. In fact, however, it does vary. We have recalculated both the ψ_s and the ψ_F values for the ranges 0-10 and upwards as projections of the 0-5 range and we find that percentage reduction increases as the range increases. For example, a projected value of 3 shows a percentage reduction of 11 in the 0-10 range and of 86 in the 0-100 range; a projected value of 55 shows a percentage reduction of 63 per cent in the 0-100 range, and of 93 in the 0-1000 range.

Within each range, however, the percentage reduction declines with increase in the size of the estimate of success (or failure) recorded. In terms of absolute numbers, the reduction is greatest when the projected number is in the region of a subjective probability of 0.5.

We may say then that at the ages studied there is no universal additive property within all ranges. The additive feature emerges most clearly within the range 0-5. At greater ranges ψ_s and ψ_F values fall short of unity; in the 0-1 range they exceed unity. This suggests that, at the ages studied, there is a tendency to write down smaller numbers than we should expect as pure projections of estimates within the 0-5 range.

Two further points of interest may be noted. First, in estimates of success we found that in both age groups discrimination tends to be finer towards the ends of the range (in the 0-100 range) and coarser in the middle. This is also the case in estimates of failure. If we combine the estimates of success and failure made by both age groups, the most preferred values are at intervals of 10, plus the values 1 and 99. Second, as in the previous experiment with estimates of success, most children thought the 0-1 range the easiest, 0-1000 the hardest, and 0-10 the range they would prefer to use in explaining to a friend.

CONCLUSIONS

(1) Estimates of failure made by children aged 9-14 years in undertaking a given task are dependent on the range within which they are made. The greater the range, the smaller relatively, are the estimates. Unlike estimates of success, estimates of failure seem to remain constant between the ages of 9 and 14 years.

(2) Under certain conditions, the additive theorem appears to hold true of subjective probability as it does of mathematical probability. ψ_s and ψ_F values sum close to unity when they are based on estimates made within the range 0-5. Within the range 0-1 they exceed unity and at ranges greater than 0-5 they are increasingly less than unity. It is possible that a closer approximation to unity might be obtained at ranges 0-3 or 0-4. These statements apply to children of the ages studied. In so far as this departure from unity at ranges greater than 0-5 is due to the tendency to record a smaller number than would be expected on the basis of projections from the 0-5 range, we might infer that ψ_s and ψ_F values based on adult estimates would conform more closely to the additive theorem.

(3) The sum of the ψ_s and ψ_F values departs from unity most when both are approaching the 0.5 value.

(4) Within any range beyond 0-5, the actual estimates decline relatively to the projected estimates.

(5) Within the 0-100 range, the most preferred values are at intervals of 10, and the values 1 and 99.

(6) At the ages studied, the range easiest to understand is the 0-1, the hardest is 0-1000, and the one preferred for explanation the 0-10.

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A CRITICAL REVIEW OF NUTTIN'S
TACHE, REUSSITE ET ÉCHEC

BY

C. F. VAN PARREREN

University of Amsterdam

As has been often remarked, there is a large gap between American and European general psychology, which seems to be widening rather than shrinking. Part of the differences in approach can be understood from the underlying philosophic attitudes of the scientists, i.e. the positivistic outlook in America and the preponderance of phenomenology in continental Europe. But the difference is manifest in the experimental procedures as well as in the theorizing. In the United States there is a preference to start an investigation with an experimental design as simplified as is possible and to use animals as subjects, in the hope of a clear-cut decision of the problem at hand. The corresponding theories are built up in a logical-constructive way. In this methodological respect the work of the late Clark Hull seems to be taken as a model by most U.S.A.-psychologists. From the strict standpoint of method there is, indeed, little to be said against this. If we turn to the contents of American investigations, however, we too often miss the things which are relevant to human psychology and its central topic: the behavior of ordinary men in their everyday life situations. Not only the results but also the problems stem from the laboratory and one is at a loss to see how they should be generalized to the outside world. The reproach of being unreal and esoteric that was launched at associationism four decades ago, still applies to many American psychological publications.

If we take a look at European psychology, it is not difficult to find the reverse of at least two of the above mentioned characteristics of the American approach. In one respect European psychology differs to its disadvantage: theory construction is, as a rule, much more vague, even diffuse, less explicitly formulated and certainly not as formalized as in some typical American instances. But at the other hand European psychologists like to find their problems in real life situations and try hard to keep a direct and intuitive understanding of all that happens in their experiments. So, roughly said, the psychological contents of European

publications are more relevant and interesting, though from a methodological point of view they do not come up to American standards.¹

A comparison between these two ways of treating the same experimental science with regard to their ultimate fruitfulness is difficult, since there is also no agreement on the evaluation of certain auxiliary and technical procedures, like the gathering and analysis of introspective reports or the statistical treatment of data. Now in this connection I think the elaborate study by Professor Nuttin: "*Tâche, réussite et échec*"² will prove to be extremely important. The researches dealt with in this book, which covered a period of fifteen years, all concern success and failure in the fulfilment of tasks and their subsequent influences on human behavior. Since the author has made a broad and thorough study of American psychological literature, he amply discusses one of the central topics of American learning theory, the problem of reinforcement. Especially the explanation of reinforcement by such concepts as positive aftereffect and need reduction, advocated by many U.S.A. stimulus-response psychologists, is critically examined. But not only are we to a certain extent justified in calling Nuttin's problem an American one, his experimental methods too are largely modelled after American examples, in particular the procedures inaugurated by Thorndike in the thirties. So we find an "American" problem tackled by "American" experimental techniques, but performed by a European investigator within a "European" frame of thought. For, though we find in Nuttin's treatise conceptualizations that are familiar to the U.S. critics of S-R-theory (like cognitive structures) we should do his work no justice if we paid no attention to its typical European atmosphere. One of the essential characteristics of this atmosphere is the attitude of the investigator. He tries consistently to get "under the skin" of the experimental subject, attempts to recreate in himself the actions of the subjects, and thus to gain access from within to what happens in the experiment. It is from this kind of sympathetic understanding of the events that explanation and theory construction take their start.³ This attitude, which is ubiquitous in Nuttin's work, in his

¹ Of course, both in the U.S.A. as in Europe there are considerable internal differences. So we find the same controversy within each of the continents, for instance as we read in an American book the criticism of Tolman's theorizing (which certainly is more "European" than Hull's) that it lacks "even a minimal amount of formalization" (K. MacCorquodale and P. E. Meehl in *Modern learning theory*, New York 1954).

² Louvain 1953.

³ Just because of this most European psychologists are eager to learn from introspective reports by their subjects. It must be pointed out, however, that Nuttin

appraisal of minor experimental details as well as in the broad conceptions of his theory, and which characterized many notable European psychological studies in past and present, is an essentially European contribution to experimental psychology. It is conspicuously absent from the average American research, methodologically clear-cut though this may be.

Now just what this attitude can achieve if it is added to American objectivity and exactness of method, we can learn from *Tâche, réussite et échec*. For on the whole I think Nuttin's interpretations of the relevant findings concerning success and failure, not only in his own but also in many well-known American experiments, are far more convincing than those of the adherents of stimulus-response theory. But here my European bias may lead me astray; and the reader himself should judge. Anyway, I recommend the book not only to those interested in learning theory but to everybody who is working in the field of general psychology.

In the following pages an English summary will be given of what appear to be the most original parts of the book, at intervals interspersed with some critical comments, partly based on my own studies in the field of learning. But beforehand I want to emphasize that such criticism is not in the least intended to detract from the excellent merits of the study.

In his first chapter the author gives a survey of the different viewpoints from which the results of action have been treated in psychology. So from the beginning he does not restrict the discussion to learning theories with their emphasis on the "law of effect", but gives attention to the ways in which success and failure and their repercussions on subsequent behavior are conceived in psychology of personality and clinical psychology. Between these branches of psychology there are differences of opinion about the ways the behavior-result is supposed to affect subsequent action. Learning theories stress, since Thorndike, the *direct* and *unmediated* influence of the aftereffects. In clinical psychology, however, success and failure are thought of as *subjective experiences* which would be appreciated by the person on a cognitive and affective level, and would influence subsequent behavior via an adjustment of personality. Because he attempts to do justice to the latter conception also, Nuttin considers it impossible to accredit the formulas of stimulus-response psychology with general validity. The arguments he uses against the latter are well-known for the greater part and need not to be repeated here, but he emphasizes one in particular that becomes very important in the further development of the study.

as a rule derives his proof from the statistical treatment of objective data, as in the American experiments after which he modelled his own.

Looking upon behavior as a series of S-R-sequences means dissecting a large whole into minor elements and losing sight of behavior characteristics which are only present under a long-time perspective. One of the cornerstones of S-R-psychology is its explanation of behavior through former learning: S-R-sequences are repeated as a consequence of preceding reinforcing aftereffects. Now repetition of S-R-sequences, which have been followed by such positive effects, may happen within a certain limited range of behavior, but if we direct our attention to larger periods in the life of a human individual, we ordinarily see quite another development. Nuttin illustrates this with the case of a person who learns to drive a car. In the beginning his over-all results in driving are certainly negative; nevertheless he persistently returns to the car. When, however, he has mastered the activity of driving to such a degree, that he practically never fails in his manipulations and thus constantly gains positive aftereffects, we see this same person drop car-driving as an activity for its own sake. He probably will drive cars in the future, but not to the end of getting successes in driving, but as a means to quite other goals (for instance getting in his job, sightseeing, or visiting his relatives). So once positive results have been ensured, the activity that can generate them is no more resumed to arouse them anew. Repeated successes lose the dynamic value that was originally inherent in them; the activity may be resumed, but only if it gets integrated in a new behavior-pattern so that it will serve as a means to a new end. But then repetition is not caused by the former successes alone, but there is also the force supplied by the newly pursued goal (which in itself may lead to bad results again, at least in the beginning).

So Nuttin's criticism of S-R-psychology is turned by him into a new evaluation of success and failure in human behavior. According to his standpoint a positive result is not the beginning of a series of repetitions, but it rather marks an endpoint to a certain type of behavior and at the same time a starting point for another, new, development. Progressive development of new goals is for him the salient fact in human mental life; the formation of habits can be an element in this development, but it can be made responsible only for the separate links in the whole chain of conduct. In defining the general direction of this "constructive development of goals" Nuttin adopts the view held by clinical psychologists, that there is a constant effort towards self-realization in every human person. The near-by and concrete goals he pursues get their full meaning only under this developmental perspective of self-realization.

An important consequence of Nuttin's theoretical position is his denial of equivalence between success and failure on the one hand and reward

and punishment (in the way these terms are used in S-R-psychology) on the other. In the context of the latter, reward and punishment, like food and electric shock, belong to the process of satisfying organic needs and do not imply cognitive activity. Success and failure, on the other hand, suppose a goal which the subject has set for himself, i.e. a task that he is trying to accomplish. Here the goalstriving includes more than the search for tension release, for it is fused with cognitive activity. The subject knows his task to be an integrated part of his larger projects, and thus reaching or not the immediate goal interferes with his tendency towards self-realization, affects his ego-structure. So Nuttin views success and failure as ego-involved states in the constructive activity of the human person.

After this theoretical introduction three experimental chapters follow, all concerned with the result of action as *subjective experience*. From these three only Ch. II about the perception of success and failure leads to a confrontation of experimental facts with the theory proposed in the introductory chapter. Chapters III and IV give rise to interesting theoretical inferences too, but in so far as they add essentially new results to those of Ch. II, these regard cognitive theory in general, not the particular branch (of "constructive development") Nuttin advocates.

The experiments on the perception of results try to find an answer to the following question: if the results of a series of performances are communicated one by one explicitly and clearly to the subject, how, then, will be his perception of the series of results as a whole? So the subject has to perform a series of for instance 20 simple tasks; after each little task the experimenter states the result to the subject as either right or wrong. After the series has been finished the subject is asked whether his results were predominantly right or wrong and further to give an estimation of the exact number of successes. A novel feature of the experiments is the use of a scheme of sanctions prepared beforehand. Thus the statement "right" or "wrong" does not depend on the real answer the subject gave, but is chosen by the experimenter according to his arbitrary scheme. As the tasks are constructed in a way that the subject cannot check his results, for him the sanction is perfectly "real"; and several subjects getting the same series of sanctions can now be compared.

What is the outcome of these experiments? As one would expect individual differences show up in the impressions of the subjects: some give a correct or near-by correct estimation; others overrate their successes and others again overrate their failures. But the degree of variation is astonishingly high. A series of 20 tasks taking about 5 minutes in its

entirety, with 10 "successes" and 10 "failures" in irregular distribution, gave a range of estimation from 5 up to 15 perceived successes. Though this large variation in such a distinct situation might lead to the supposition of dynamic tendencies to perceptual distortion, more and other experimental data are needed to reach a decision on this matter. Nuttin provides us with quite a lot of further experimental results, though, as we shall see, perhaps not yet with conclusive evidence.

First we learn, that there is a correlation between satisfaction with results and estimation, i.e. those subjects satisfied with their results tend to exaggerate their successes and vice versa. However, one could say, because of the impression of a greater or lesser number of successes, in itself casual, the subject will be more or less satisfied with his results. —Then, over- and under-estimation of successes appears also with a reduced number of positive sanctions, e.g. 3 out of 24, or even 1 out of 8; this result is impressive but not yet conclusive. A further experimental check is then introduced: Nuttin gives his subjects a mixed series of 10 additions and 10 subtractions. Every item is on a separate slip of paper, and after completion of an item the subject has to put the slip into one of two boxes, marked + and —. There is no result communicated to the subject, but here the aftereffect consists in the subject realizing whether he made an addition or a subtraction. Now the questions the experimenter asks after the series concern the number of additions the subject made. So there is a close parallel with the estimation-of-results experiment, but since the number of additions can be assumed as affectively neutral to the subject, an opportunity is given to inspect the extent of sheer inaccuracy of judgment under the experimental conditions. In fact, the interindividual variation is smaller here than with the estimation of results; but it is still considerable. However, and this is important, even in this "neutral" situation over- and under-estimation are not the outcome of random inaccuracy of the subject, but can be ascribed at least partly to a systematic factor, namely the structure of the series. If a series begins with a majority of additions, the impression of predominance of additions in the series is the rule and vice versa with a series starting in the main with subtractions. This influence of the structure of the series is, afterwards, examined with the estimation of results too. Since Nuttin worked with predetermined schedules of sanctions, it was easy to study the effect of a series wherein positive c.q. negative sanctions predominate in the beginning, the total number of each type of sanction in the whole series being kept constant. Here again the structural factor as a cause for distorted perception could be demonstrated.

So far no point of contact between these findings and Nuttin's preceding theoretical exposition is obvious. The demonstration of the structural factor is interesting, but one should note that this factor is a purely cognitive one and that it bears no clear relation to ego-involvement or constructive development of goals. Nuttin himself is of this opinion, for the experiments with neutral phenomena (like addition and subtraction) are explained by him on the basis of structural factors alone. Every time, however, he finds an incorrect perception of the *results* of the subject's performances, he claims other, not purely cognitive but personality-determined factors to be at work beside the influence of the structure of the series. I cannot find his arguments for this interpretation very convincing, for I see only two groups of experiments in which the invoking of dynamic factors is inevitable. In one experiment he worked with selected groups of subjects. Teachers, guided by an elaborate instruction, selected "optimistic" pupils ("living in an atmosphere of success") and pessimistic ("living in an atmosphere of failure"). Further he used psychiatric patients, namely mild or nearly recovered cases of mania and melancholia. With these subjects a correlation was found between depression and underestimation of successes on the one hand and elation and overestimation on the other. In a second experiment Nuttin could demonstrate that the degree of ego-involvement of the subjects with the tasks to be performed influenced the direction of the perceptive deformation of the results.

Having reviewed the principal experiments of Ch. II of *Tâche, réussite et échec*, we must discuss their theoretical significance. The following seems to be convincingly proven:

- a. There regularly occurs a perceptual distortion of the results of the subject's own action, even if these results are clearly communicated to him; this distortion may either overaccentuate successes or failures.
- b. The cause of this distortion may lie in the structure of the experimental series, i.e. the distribution of successes and failures within the series.
- c. Beside this there may also operate factors at the dynamic or ego-level of the person which can cause distortion in either direction.

Now let us see what the author has to say in his section on "theoretical interpretations".⁴ The subject, who enters upon the series of tasks has already an attitude, explicit or not, towards his possibilities of performing it. This attitude implies an expectation regarding the frequency of occurrence of successes and failures; the true frequency of these may be distorted

⁴ Pages 145 seq.

so as about to conform with the expectation. If an expectation is to have this effect, it cannot be purely cognitive but must be accompanied by the *need* to maintain it on its once established level.⁵ This need is the need for maintaining a general level of achievement by the subject (K. Hoppe's ego-level) and this need is rooted in the need for self-consistency, i.e. the tendency of the ego to preserve a certain once-formed concept of itself. So the perceptual distortion that may undergo the results of performances must be interpreted, according to Nuttin, as a mechanism of adjustment of the ego akin to the well known perceptual defense.

This elaborate explanation sounds plausible, but does it find sufficient support in the experiments? Naturally the explanation does not have its foundation in Nuttin's experiments alone, but in the array of researches Nuttin refers to, concerning level of aspiration, interruption of activities, transference of affect, the self-concept and the like. But still we have some questions to pose. First, can the explanation account for the overrating of *failures* by certain subjects; and then: is there any relation between it and the theory of constructive development forwarded in the introductory chapter?

Both these questions are dealt with by the author. As to the first, he points out that just like exaggerating one's successes in one's own eyes, also minimizing them may be a way to preserve one's self-concept. In the latter case the self-concept derives its value for the person from the stress which is laid on the ideal person one should like to be. Just by disparaging his real results such a person is able to emphasize more his aspirations, and, therefore, to attach value to his personality.

The discussion of the second question seems to me less satisfactory. From the view of man as involved in a dynamic development towards self-realization one can draw different conclusions with regard to how he will face the results of his own performances. One could suppose that the mature "constructive" man would accentuate his failures; for successes lose, through repetition, their dynamic value.⁶ Indeed Nuttin considers people who constantly return to their former successes as infantile in this respect, just because they have not reached the higher, constructive stage of development. On the other hand it is also a sign of maturity that one can stop troubling about his failures if they are irreparable, like those in the experiments under discussion. So our conclusion should go the

⁵ For if the subject's expectation of a certain frequency of successes would be a cognitive affair only, the not-expected failures would rather be accentuated instead of overlooked.

⁶ See p. 384.

opposite way: in "normal" behavior there ought to be an accentuation of successes. But further we could ask whether mature behavior should not imply a more *objective* attitude towards the results of one's actions. If this is true, ought there to be in well integrated, normal subjects any perceptual deformation at all? These possibilities are all taken into consideration by Nuttin, but we are not told his final opinion on this matter. This omission and a corresponding one in the experimental programme, namely the circumstance that Nuttin did not examine the age- and maturity-influence on the perceptual distortions systematically, are no doubt serious drawbacks of this part of the book. Still we can learn much from it, for, after all, we have been placed in possession of facts concerning the influence of cognitive structures on the perception of results and the coordinate influence of ego-induced tendencies. The author's outlook on mature human behavior is neither supported nor contradicted by his experimental data; and to the discussion we owe many stimulating insights.

We now turn to Part II of *Tâche, réussite et échec*. The introductory chapter of this part of the book makes very interesting reading, as the author gives an uncommonly penetrating sketch of the historical development of Thorndike's ideas. In Ch. VI he resumes the account of his experiments and here he deals with the question whether a success, or the need reduction which is its consequence, necessarily reinforce the connection between situation and response. The first experiment to answer this question was designed as follows. The subject is confronted with a series of pictures, every picture showing in one context or another a large number of the same kind of objects, for instance a flock of sheep, a block of houses, an avenue with trees, etc. For every picture the subject has to give his estimate of the number of objects, within 4 seconds; and thereafter the experimenter pronounces his sanction "wrong" or "right" purportedly with regard to the approximation reached by the subject, but actually again according to a preestablished scheme comprising equal numbers of successes and failures. After the subject has completed the series the experimenter surprises him with the task of recalling one by one his estimates. So the experimenter asks for the number of sheep, of houses, of trees, etc. the subject has mentioned.

This kind of experiment was performed with 321 subjects, who gave a sum total of nearly 9000 responses, half of which were sanctioned positively, the other half negatively. The crucial question is if there is any difference in the reproduction of the responses followed by "right" and those followed by "wrong". Nuttin calculates a mean percentage of

48.1 for the first category as against 48.3 for the second. He concludes, therefore, that within the performance of a task success in itself does not necessarily lead to better learning of the responses it belongs to.

At first sight, this result is astonishing as it contradicts not only need-reduction theories of reinforcement, but also seems to be in conflict with well-established experimental results (like those of Thorndike's) where the "right" and "wrong" from the experimenter evidently secured a learning process.

To settle the conflict one could try to consider it as a matter of different criteria for learning. Nuttin uses *reproduction* of the original response as a learning criterion, whereas in the ordinary experiment learning is inferred from *repetition* of responses. However, an experimental test does not confirm this supposition. For this purpose Nuttin adapted the well known Thorndike experiment with Spanish words. He let the first trial take its normal course, but then changed instructions to the subject. Instead of continuing the learning task with more trials, as the subject would expect, he asked simply to recall his responses from the first trial. In this experiment a significant difference was obtained in the reproduction of responses judged as right during the first trial and those judged as wrong. So differential learning of right and wrong responses may be demonstrated with Nuttin's method of reproduction as well as it may be inferred from increased or diminished repetition. Thus, we have all the more reason to assume that right and wrong in the estimation experiment have no influence on learning.⁷

How, then, are these controversial results to be explained? Here the author introduces a most interesting and consequential pair of concepts, namely of *open and closed tasks*. In the estimation experiment the task with which the subject is confronted is: giving a number-estimate for each card; once the estimate has been given, the special task for this card has been fulfilled, and the subject has no more to do with it. Nuttin calls this

⁷ One could wonder why Nuttin uses the reproduction method at all instead of adopting the ordinary procedure. Of course he could have asked his subjects for a second estimation of every card, in order to inspect repetition of the first estimate during the second trial. But in this case there would be, as Nuttin points out, besides a possible difference in the strength of connections, a difference in motivation regarding the right and the wrong responses. Differences in the number of repetitions during the second trial could be explained by the motivation of the subject to make as many correct estimations as possible. The subject might therefore deliberately repeat the response called right and omit those called wrong, without a difference in the strength of connections *per se* existing. If the subject's task is: reproduction of all the responses from the first trial, the motivation for both categories of responses is equal. So differences that show up in this case must be due to differences in the strength of connections, i.e. to differential learning of right responses as against wrong ones. (For the rest Nuttin finds an argument *ad hominem* in the fact that Thorndike himself used reproduction as a criterion for learning in other experiments. See Nuttin, p. 310.)

sort of tasks *closed* ones. In the Spanish-words experiment the subject's set comprises more; apart from giving responses during the actual trial, he wishes to augment the number of right responses when the same items will be passed to him in subsequent trials. So any single response does not bring to an end a part of his task, but the task keeps essentially an *open* one. With such an open task, the results of the subject's responses acquire two aspects. One is the aspect "sanction"; it bears upon the response which has just been given. The other aspect, "information", on the other hand, concerns the task in so far as it is still to be performed: through the announcement of the result the subject is informed for the future whether a certain response will suffice for a given item or not. Now it is clear that the first aspect constitutes the need reduction (if the sanction is positive), and further that only with an open task the result takes on both aspects. For with a closed task the subject cannot be informed with a view to the future since with a closed task there is no such future.

By means of these concepts Nuttin frames his explanatory hypothesis like this: strengthening of connections during the fulfilment of tasks can only occur if the task is an open one, for in this case the results of the subject's responses have an informative aspect. Need reduction proper does not strengthen connections; on the contrary, it is "need persistence" (as it exists in open tasks) that secures the strengthening, i.e. the learning process.

This hypothesis is tested by Nuttin in several experiments from which I will cite only one. In this experiment the influence of repeated application of positive sanctions to the same connection is tested in a situation of closed task. The absence of any influence of the positive sanctions in the number-estimation experiment cited above, might be caused by the circumstance that the sanctions were applied in only one trial, which means for every connection only once. Possibly the influence could have stayed subliminal. To test the cumulative effect of positive sanctions in a closed-task experiment requires an uncommon experimental set up, because repetitions of sanctions will normally bring about a situation of open task for the subject. Therefore, Nuttin devised an experiment along the lines of Thorndike's learning without awareness. The subject is confronted with a long series of cards. This time two big letters have been drawn on each card, and the subject has to judge which of the two occupies the larger black surface. Since the drawings have been made in such a way that on each card these surfaces are approximately equal, it is possible to introduce a preestablished scheme of sanctions without rousing suspicion in the subject that he is performing anything else but a series of individual

judgments. So indeed the subject's attitude is one of closed task. Yet in this design a possibility for strengthening connections has been concealed. In a series of 144 cards for instance there are only 12 pairs of letters, each pair recurring 12 times. As each of the 12 cards with the same pair of letters shows them in another type and since in the series as a whole the 12×12 cards are mixed in an arbitrary way, the individuality of the items for the subject is warranted. Now the scheme of sanctions consists in applying always "right" to the judgments of the 6×12 cards showing six pairs of letters, and always "wrong" to the other half of the cards, showing in each case one of the other six pairs. The subject has to pronounce every judgment aloud, like "R is larger than B", or "R is smaller than B", and then the experimenter announces his "right" or "wrong". Thus, at the end of the series, the sequence R—B for instance has been followed 12 times by the sanction "right"; another sequence, like Z—N or so has been "punished" 12 times by the sanction "wrong". Then the subject is given a reproduction task: he must fill in the second letter of each pair when the first one is presented to him by the experimenter.

The essential question of the experiment will be clear: is there any difference in the number of reproductions of the letters which always led to a good result and those that led to bad results? The prediction from Nuttin's hypothesis is, that there will be no difference. Because the subject lacks the open-task attitude the repeated sanctions should have no influence on learning. Need-reduction theory, on the other hand, will predict a differential strengthening of the connections between the letters. Now with a total amount of 444 subjects giving 6304 responses no significant difference was found, which surely means a new and strong argument in favour of the open task hypothesis.

From the second part of the book some other experiments will be discussed, but first I pass on to the final chapter of *Tâche, réussite et échec*, called "Elements of a theory of human behavior". Here the author sums up his experimental results and theoretical discussions and extends them to a broad view of human behavior. In this paper it is not possible to review adequately this long and rich chapter, so I restrict myself to such points as are closely connected with those selected from the preceding chapters.

In the first place one can ask how Nuttin relates his findings concerning the open task with his viewpoint of the constructive development of human behavior. His reasoning runs something like this. S-R-psychology attributes

to success, reward, or positive sanction (between which it does not distinguish) a direct and unmediated influence on the strengthening of connections. So the development of behavior would depend on the incidence of the sanctions. Motivation would merely have an extrinsic relation to this development, viz. in so far as it makes possible the occurrence of positive behavior-results (through the "reduction" of needs). If we, on the contrary, accept the open-task hypothesis as valid, then we must deny the behavior-results an unmediated influence. For, if a behavior-result will have any influence at all on subsequent performances, it must be informative; and it can only be so if the subject is set for an open task. Precisely, it is the intrinsic, cognitive, relation between behavior-result and motivation, called information within an open task, which is necessary for learning. Further, an open task implies a persistent motivational system behind it; for "openness" means openness to the future. If this future-perspective lacks, i.e. without the existence of such "need persistence", the subject will abandon a certain line of conduct at the same time as its performance has finished, regardless of its results. On these grounds Nuttin paraphrases the learning in an open task situation as learning through cognitive incorporation in a dynamic system of the person.

That a behavior result has no "blind" strengthening force, but is only effective if it carries such information that it will be incorporated into one of the person's persistent dynamic systems, can be seen from Nuttin's ingenious experiments on the incorporation of "punished" responses. In these experiments he creates a situation for the subject in which the response which is followed by the "wrong" from the experimenter will somehow fit in with the subject's dynamic system, whereas the correct responses have no such value for him. In this case indeed the wrong responses are better learned than the right ones. In one of these experiments the subject guesses a number supposedly belonging to a letter which the experimenter calls to him. After every guess a sanction follows, positive or negative according to a preestablished scheme. Beforehand, the subject has been informed that if a guess would be right, the letter would be no more repeated to him, but after "wrong" a new guess would be asked later on. In the latter case however, the experimenter would provide a cue by telling the difference between the old, wrong, guess and the correct answer. So a response called right in this experiment, could be abandoned by the subject; but if a response would be followed by wrong, it would be of value in the fulfilment of the (open) task. In this and similar experiments a significantly better reproduction of wrong responses as compared with right ones was the rule, when the experiment was stopped after the first trial and reproduction of all responses was asked from the subject.

However important these experimental conclusions may be, it seems again that a comprehensive theorem like "constructive development of human behavior" does not follow from them. But at least it seems that the road to it has been freed from obstacles. Always assuming the above

reasonings to be compelling (about which I shall say something later on) one must conclude that S-R-theory, which leans on need-reduction learning in the explanation of behavior-development, fails to picture the role of motivation in this development adequately. Human motivation, fundamentally influenced by cognition, must have the last and decisive word. So far as the development of behavior could be ascribed to learning, it is always a learning dependent in its directions on personal motivation. Thus the rigid scheme of S-R-theory has been broken and there comes room for the possibility that human motivation (and with it behavior as a whole) goes through a kind of development, which Nuttin calls "constructive".

Thus our next step will be to take note of Nuttin's opinion about the ways in which human motivation develops. Here he has no experiments of his own to take as a startingpoint, and we see him endorse the principle of canalization: Apart from instinctive motivation, there are no innate behavior-patterns in which needs manifest and realize themselves. They take on a concrete form only through contact of the organism with its environment, i.e. through learning. This learning process means the canalization of the originally vague and undifferentiated need into a regular behavioral scheme.—With some sort of a surprise we read that Nuttin in adopting the concept of canalization also accepts its common explanation by need reduction: The responses that succeed in reducing the need leave enduring traces and are absorbed into the behavioral dynamism of the need itself.

Some critical notes seem necessary to me on this point of the argument. Nuttin acknowledges two forms of learning. One on the ground of need persistence, implying a cognitive appreciation of the need by the person together with a cognitive incorporation of the responses in the need system. The other form of learning, by need reduction, is expressly denied a cognitive aspect. What one would like to know is: from which condition does depend the occurrence of the one and the other form? Here the author offers only some rather unsatisfactory remarks. Certainly he does not refer to the nature of the subjects alone, i.e. need-persistence learning only to occur in man, need reduction operating in animals. For, though he leaves open the possibility that with lower animals there might be no cognitive elements in the learning process, he states explicitly that in human behavior need reduction can secure learning. His addition that the results of need reduction in his own experimental conditions is "quasi nil" does not prevent him to suppose every time two learning processes at the same time in the human organism, one at the cognitive level, one in

the form of canalization. Only their relative importance would vary in individual cases. As, however, no facts or even hypotheses are given concerning the relevant causes operating in the supposed double learning process (in which cases, for instance, will need reduction be demonstrable at all in human learning?) the whole idea of this doubleness is an inconclusive one, and, by the way, one which makes little of scientific parsimony.

Yet another objection seems still more serious to me. In Nuttin's opinion learning, also in its cognitive form, is dependent on motivation: But from his discussion of the canalization process we gather that motivation will mostly take its concrete form through a process of need-reduction learning. Are not we here confronted with a sort of Münchhausen process, i.e. dragging oneself from the water by pulling one's own hairs? Because first we have been told that learning, even in its cognitive form, is always predetermined and guided by motivating forces; and then, these motivating forces would acquire their concrete, behavioral form through learning processes. Here is a logical fault in the system. But, then, there is a psychological defect too. The explanation of a constructive development of motivation cannot rely on any such blind process as canalization. One looks for a way out in other directions and, involuntarily, is reminded of Allport's functional autonomy of motives. How much is one surprised finding Nuttin, near the end of his book, as an adversary of Allport's conception, just as of Woodworth's "mechanisms become drives" formula. If a mechanism apparently becomes a drive—Nuttin's statement runs—it has only switched over from one driving force to other preexisting ones. This is a well-known interpretation, but it hardly fits in with Nuttin's further notions. Moreover, the Allport-Woodworth principle is not replaced by any other, from which motivational development could be derived.

The discussion of *Tâche, réussite et échec* could end here, but I prefer to try and suggest a small contribution to the solution of these difficulties, in which the reader of this excellent investigation finds himself. For that purpose the first thing we need is a diagnosis: what causes the difficulties in the system? In my opinion it is the denial of the *independence of the cognitive sphere*. As appears from the foregoing, Nuttin is both a "cognitivist" and a "motivational monist". But these two affiliations are hard to combine. If one introduces cognitive processes into one's psychological system, it appears to be difficult to maintain them as "ancillae" to motivational forces. Only by admitting independent cognitive processes, i.e. not predetermined in their actual course by motivation, difficulties like those in Nuttin's system seem to be solvable. An indirect conformation

of this view can be found in influential cognitivist systems, like that of Tolman's, which indeed accept purely cognitive learning processes without the necessary interference of motivation.⁸ Once we accept the idea of cognitive processes which are not guided by or perhaps even instigated by present motivating forces, we can understand the possibility of future motivational development; for this may result from the outcome of the cognitive processes. I do not think this proposal far-fetched (at least if one accepts the operation of cognitive processes at all). For man invents many things which he certainly did not intend to discover at the outset, so he invents without being motivated for his invention. But once the invention is present, it may arouse his motivation, and a *new* motivation at that; a process which is illustrated in a concrete form with all sorts of technical discoveries. Here seems a real possibility for new motives to emerge, but it is only there if one does not restrict the operation of cognitive processes to a scope which is prescribed by motivating forces of the past.

The autonomy of the cognitive sphere, here advocated, is more in the nature of a postulate than of a demonstrable hypothesis. None the less one can try to find facts which show the postulate to be a sensible one. It is important that we can find experimental results, pointing in this direction in this very book of Nuttin's. From the experiments in question can be inferred, that even with learning in a narrower sense (i.e. not including problem solving or any creative invention) cognitive structures have a determining influence without motivation necessarily being involved. May this inference prove correct, then the postulate of autonomy of cognition seems very tenable indeed; for if cognitive structures appear as autonomous in learning proper, it would be queer when cognition in its more creative manifestations would be the handmaid of motivation.

One of the experiments which may be considered in this context has already been reviewed. It is the closed-task experiment with repeated sanctions.⁹ I mentioned as its result, that no significant difference was found between letter-sequences followed by "right" and those followed by "wrong". If we now turn to the exact values involved in the comparison, then we see that they amount to 40.4 and 42.0 per cent correct reproductions respectively. This means that, although the task was a closed one to the subjects, they learned quite a lot. It is evident that this learning result cannot be explained by the two principles accepted by Nuttin. For

⁸ It is only consequent that Nuttin objects just to this point of these theories. See for instance Nuttin, p. 441.

⁹ See p. 391.

these correct reproductions cannot be ascribed to need-persistence learning, since this occurs only with open tasks; nor can need-reduction learning have played a part, since this would have led to differential reproduction of right and wrong sequences. The only possibility that remains is to accept a form of learning which is not motivation-induced.

That this form of learning may be properly called autonomous cognitive learning seems to be proven by a modification of the above experiment which is also reported by Nuttin. Whereas in the original experiment it was the surfaces of the two letters themselves that had to be compared, in the modified experiment the subject compared the surfaces of two geometrical figures. But now a letter had been drawn over each figure, in smaller type than in the original experiment but still more than an inch high. The subject was instructed to indicate the figures by calling the letters. So his judgments sounded exactly alike in both experiments, like R larger than B, Z smaller than N, etc. All other experimental details were left unchanged; so again there were six pairs of letters that got "right" after each of their 12 pronunciations, and six others that continually got "wrong". Again no significant difference in the reproduction of positively and negatively sanctioned letter-sequences was found (as in the original experiment), but here it is essential that there was no significant degree of reproduction at all, i.e. correct reproductions occurred no more than could be ascribed to chance. Nuttin considers this result first as a confirmation of the rule that sheer contiguity does not secure learning; secondly, again he concludes that a sanction is ineffective if it is not given in an open-task situation. But then he explains the divergent result of the original experiment through the structural unity in the perception of the pair of letters; a unity which does not come into being in the process of the modified experiment. In the latter the comparison of the surfaces brings about a structural unity of the pair of surfaces, but not of the letters, which are, in that case, merely externally affixed elements. So Nuttin admits that whether a learning process occurs or not may be dependent, not on motivational factors (which are the same in both experiments), but on a perceptual structuring of the situation, i.e. a cognitive process.

The same sort of argument could be based on quite a series of other experiments in which Nuttin shows that no differential reproduction results from sanctioning positively or negatively in a closed-task situation, but where learning took place to a considerable degree irrespective of the nature of the sanctions. A most convincing example I find in an experiment inspired on the well-known Köhler-Von Restorff investigation. The subject must again compare surfaces, this time of letters or digits. So the task is

closed and here no sanctions at all were given. Now learning was considerable, to a nearly perfect degree, if an isolated pair of digits was exposed within a series of letters or vice versa. The influence of cognitive structures has been demonstrated with this experiment in a purer form than in the Köhler-Von Restorff design, since these authors gave their subjects a learning instruction, i.e. operated with open tasks in Nuttin's terminology.

My closing remarks regard Nuttin's definition of learning. As "the fundamental principle of learning" he considers the *integration of responses in the subject's dynamic system*. This definition comprises both need-persistence and need-reduction learning. In the first case the integration of responses is procured by the process of cognitive incorporation within an open task. In the second, it is the non-cognitive process of canalization that can be labelled as "integration in the dynamic system". Nuttin's standpoint accepted, his summarizing of both forms of learning under one heading is logically correct. But in my view three objections can be raised against the definition. First: it has been shown in the above, that with a closed-task attitude the subject's responses may still be learned, irrespective of the behavior-results. One is at a loss to see how in this case anything could be integrated in a dynamic system, as the dynamic system in question does not persist, nor is there always a need reduced. Secondly: the definition considers learned responses as responses integrated in a dynamic system within the person. But are not there learned responses, which are not related to any specified dynamic system at all, but which are at the disposal of every such system that could possibly present itself? Nuttin's own example of the technique of car-driving can illustrate the point. Once a person has learned to drive a car he can use it as a means to very different ends, i.e. he can insert it deliberately into any dynamic system. Yet the learned responses themselves have no home in any particular system. Suppose the man we are talking about cannot procure himself a car during a period in his life and he makes use of other conveyances. The responses necessary for car-driving have in this period no dynamic value at all, still they are largely retained, as appears when perhaps after years, the man drives a car again. The third objection against Nuttin's definition emerges if we accept the foregoing two. In that case we must drop every reference to motivation from the definition, but then the question pops up, what remains? I propose that a definition or fundamental principle of learning ought to tell something about the way in which responses are elicited and performed after they have been learned. A learned response differs in its general character from non-learned ones,

like for instance impulsive reactions, haphazard tries or carefully thought-out novelties. Such an intrinsic feature of learned behavior lacks in Nuttin's definition.¹⁰

At the end of this discussion I should like to warn its readers not to conclude from it that Nuttin's book gives one more to criticize than to learn from. To me it seems one of the most important books that have been written on learning, but, as I remarked in the beginning of this paper, the reader himself should judge.

¹⁰ On the basis of my own researches I proposed as such a distinguishing feature the *perceptualization of responses* in learning. An account of this view on learning has been given before (this journal 1954, 10). Through confrontation with Nuttin's investigation it appears that this perceptualization theory of learning would have less difficulties with the explanation of his data, since it conceives learning as an essentially autonomous cognitive process (i.e. independent of motivation) which still can take up motivational influences (like for instance in open-task learning). Further it is not necessary to state a double learning process in the way Nuttin suggests, as in perceptualization theory learning is considered as ordinarily stratiform.

COMMUNICATIONS OF THE INTERNATIONAL UNION OF SCIENTIFIC PSYCHOLOGY

NEWS AND NOTE

SWITZERLAND

As a result of a grant from the Rockefeller Foundation, the "Centre International d'Epistémologie génétique" was recently established in the Faculty of Science, University of Geneva, under the direction of Jean Piaget. The Centre, which was opened on October 1, 1955, is an interdisciplinary one, and is closely associated with the Department of Psychology, University of Geneva. It consists of a team of research workers (from the logico-mathematical, natural and psychological sciences) interested in the experimental and theoretical study of developmental behavior. Its members are L. Apostel (logic and axiomatisation in behavioral sciences), B. Inhelder (child development), B. Mandelbrot (language, decision theory and information theory), W. Mays (logical machines and cybernetic models), A. Morf (research on mental development of logical reasoning), J. Piaget (genetic psychology), and J. Rutschmann (genetical evolution of perception and physiological mechanisms). The general topic of study for the present year is the relation between logical structures and the behavior and thought of the subject in his development. A guest Symposium on this topic was held in July, 1956, when the first publication of the Centre also appeared.

Inquiries and correspondence should be addressed to the Acting Secretary, Mlle. S. Taponier, Centre International d'Epistémologie génétique, Geneva 14, Switzerland.

UNITED KINGDOM

F. C. Bartlett has been invited by the Royal Society to give the Croonian Lecture. This is the oldest lecture foundation of the Royal Society, founded in 1684, and effective since 1701. As far as is known, Bartlett is the first psychologist ever to be invited to give this lecture.

Dr. N. H. MacWorth delivered the Sir Alfred Herbert Lecture for the Institute of Production Engineers at the Royal Institute of London on February 9, 1956. The title of his lecture was "Work Design and Training for Future Industrial Skills".

NECROLOGY

Marcel Griaule, Professor of Ethnology of the Faculty of Letters at Paris, passed away in March, 1956.

M. Delvaux, Director of the Farm School in Waterloo, Belgium, who was serving as Treasurer of the General Organizing Committee of the 15th International Congress of Psychology, died suddenly during the month of March.

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